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ABSTRACT

This volume contains twenty-five of the papers presented at the Seventh Algonquian conference. Topics covered in the papers include recently discovered linguistic fragments of Ocanahowan, the Wittiko people, ethno-history and archeology of the Mushuan, color terms in Narragansett, the Christian holidays of the Wabanaki, dialects of the Eastern Ojibwa-Odawa, historic populations of northwestern Ontario, Cree myths, unspecified-subject phenomena in Algonquian, southern New England shamanism, the coding of role information in Ojibwa, physical anthropology in northwestern Ontario, Eastern Abnaki shamanism, the role of warfare among the Southern Algonquians, lexical symbolization and phonological processes in Delaware, a comparison of traditional Cree, acculturated Cree and Euro-Canadian values in children, Montagnais dialectology, the Proto-Algonquian Urheimat, developing an orthography for Micmac, territorial distribution of the Western Woods Cree, Algonquian languages in Indian education, style in social relationships, and the seventeenth-century Wabanaki Sagamores. (CLK)

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# PAPERS OF THE SEVENTH ALGONQUIAN CONFERENCE, 1975

Edited by

William Cowan

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# INTRODUCTION

The Seventh Algonquian conference was held October 31-November 2, 1975 at Niagra-on-the-Lake. It was organized by Dr. Richard Preston of McMaster University, and like previous Algonquian conferences was a rich, exciting mixture of the history, archaeology, ethnology and linguistics of the Algonquian-speaking peoples. One is continually amazed and fascinated by the interest that this group of cultures has sparked in the scholarly disciplines, not only of today but of the past. As more and more of the history of the confrontation and interaction of European and Amerindian cultures comes to light, from Roger Williams in 1643 through Capt. John Smith, the early Hudson's Bay traders, 19th century missionaries, up to the adoption and adaptation of TV by modern Crees, we begin to get a picture of how these two cultures have formed one another and how, for better or for worse, we are what we are because of this encounter. One of the tasks of Algonquian scholarship is to document and anlayse this process and these results, and the present collection of papers is an outstanding contribution to this effort.

Thirty-five papers were presented during the conference, and one paper, missing due to the mail strike, was submitted later. Of these 25 were submitted for publication. The papers not submitted are the following:

Charles Bishop: The Henly House massacres and Hudson's Bay Company justice

Calvin Martin: The fur trade and early 17th century Micmac settlement patterns

John Nichols, et. al.: Northwestern Ontario dialect survey research report

Ives Goddard: Remarks on the locations of tribes of Michigan before 1650

John Hamer and Jack Steinbring: TV adoption among Cree and Salteaux of Manitoba

Gordon Day: The Western Abnaki transformer

James Axtell: The unkindest cut of all, or, who invented scalping?

Brian Craik: Space and control in Cree childrearing

Arthur J. Ray: Economic anthropology of the sub-artic fur trade: the search for a suitable methodological approach

John Nichols: Potawatomi traditional writing

James V. Chism: An overview of the ethnoarchaeological field research in James,Bay, Québec

It is a pleasure to present the papers that are included here, and it is a pleasure to reflect on how far we have come, in linguistics at any rate, from 1964, when Don Deblois stated that there were 12 linguists in all of North America actively engaged in Algonquian linguistics. There must be many dozens by now.

William Cowan
Carleton University

# OCANAHOWAN AND RECENTLY DISCOVERED LINGUISTIC FRAGMENTS FROM SOUTHERN VIRGINIA, C. 1650

Philip Barbour
Ridgefield, Connecticut

Ocanahowan (or Ocanahonan and other spellings) was the name of an Indian town or village, region or tribe, which was first reported in Captain John Smith's True Relation in 1608 and vanished from the records after Smith mentioned it for the last time 1624, until it turned up again in a few handwritten lines in the back of a book.

Briefly, these lines cover half a page of a small quarto, and have been ascribed to the period from about 1650 to perhaps the end of the century, on the basis of style of writing. The page in question is the blank verso of the last page in a copy of Robert Johnson's Nova Britannia, published in London in 1604, now in the possession of a distinguished bibliophile of Williamsburg, Virginia. When I first heard about it, I was in London doing research and brushing up on the English language, Easter-time 1974, and Bernard Quaritch, Ltd., got in touch with me about "some rather meaningless annotations" in a small volume they had for sale. Very briefly put, for I shall return to the matter in a few minutes, I saw that the notes were of the time of the Jamestown colony and that they contained a few Powhatan words. Now that the volume has a new owner, and I have his permission to xerox and talk about it, I can explain why it aroused my interest to such an extent.

To begin with the end, so to speak, I would like to expatiate on the name Ocanahowan and its history, and then proceed to a conclusion involving the Powhatan words. That is a subject on which I profess almost total ignorance. As I have said, Ocanahowan was first mentioned in John Smith's True Relation, and, as I have not yet said, it first appeared on a map in the same year. This was the map that was sent, along with a letter, to Philip III of Spain, by his ambassador to James I, Don Pedro de Zúñiga, one of whose duties was to keep Philip informed of all threats to Spain's predominance in the regions west of approximately 50° west longitude as established in 1494 by treaty with Portugal.

John Smith's first references to Ocanahowan (I have arbitrarily chosen this spelling for the name) are brief. (Barbour, vol. 130: 182 ff):

1) Powhatan's half-brother Opechancanough "spared not to acquaint" Smith with word of "certaine men cloathed [like me] at a place called Ocanahonan," which has been corrected in pen and ink by an anonymous annotator to read "certaine men at a place 6 dayes jorny beyond Ocanahonan".

2) Shortly thereafter, Smith sent a letter to inform the fort at Jamestown about "Ocanahonum".

3) Powhatan himself described to Smith "the people cloathed at Ocamahowan," and also confirmed what "the rest" had said, "that reported us to be within a day and a halfe of Mangoge, two dayes of Chawwonocke, 6 from Roonock [Roanoke, Ralegh's colony] to the south part of the backe sea [the Pacific Ocean]..."

The map I have mentioned, which I have called the Smith/Zúñiga map for obvious reasons, adds further details. It shows Ocanahowan on the south side of a river which must be the Roanoke some distance above its confluence with the river now called the Chowan. In addition it supplies descriptive notes along with other place-names:

1) There is a town called Pakerikanick on the second important river to the south, next to which is a note: "Here remayne the 4 men clothed that came from Roonock to Okanahowan;"

2) north of this river (the modern Neuse) a larger river is shown (the Pamlico) and between that and the Roanoke there is another note: "here the king of Paspaheygh reported our men to be and went to se[e]" (note that Paspahegh was always inimical to the English);

3) still more to the north, at Warraskoyack (on the modern Pagan River),

is a third pertinent note: "here Paspanegh and two of our men landed to go to Panawiock" (located next to the preceding note).

Here, then, we have a clue to the interest in Ocanahowan. The Englishmen mentioned were said (or thought) to be survivors of Sir Walter Ralegh's Roanoke colony, which withered on the vine, or was rudely uprooted, sometime between August 27, 1587, when John White sailed from Roanoke, and August 18, 1590, when White returned and found the colony deserted. Whatever the facts, the immediate cause of the severance of the lifeline to England was the Spanish Armada of 1588. Not even Ralegh was able to care for the colony at the time. Still, despite the passage of the years, not everybody forgot the men and women left behind, or the rumored copper-mines of Chaunis Temoatan, up the Roanoke River, above Ocanahowan.

But even while the Roanoke colony flourished, briefly, its site had never seemed the best possible, and Ralph Lane had visited and reported favorably on the southern shores of Chesapeake Bay (unfortunately, very little is known about this visit). So, when the Virginia colony was being planned in 1606, the site proposed was the lower Chesapeake Bay region--not Albemarle Sound. At the same time, however, the colonists who were to found Jamestown were instructed to look for survivors from Roanoke Island, and to explore the whole area further for silk-grass, copper mines, and so on.

Though there are no known surviving instructions to this end, there must have been verbal orders of some sort. In his True Relation Smith wrote that sometime in late March or early April, 1608:

> We had agreed with the king of Paspahegh, to conduct two of our men to a place called Panawicke beyond Roanok, where he reported many men to be apparelled [like us]. We landed him at Warraskoyack, where [he] playing the villaine, and deluding us for rewards, returned within three dayes after, without going further.

Then, toward the end of 1608, when the chronic disorders and mutinies in Jamestown simmered down a little, Smith himself sent a scout southwards from Warrascoyack, on December 30 or 31. In the words of the *Proceedings*, he asked the werowance of that tribe for guides to Chowanoke, explaining that:

> He would send a present to that king to bind him his friend. To performe this journey was sent Michael Sicklemore, a very honest, valiant and painefull souldier, with him two guids, and directions how to search for the lost company of Sir Walter Rawley, and silke grasse.

To this passage, Samuel Purchas added a side-note when he reprinted it in his *Pilgrimes* (1625):

> Search for them sent by Sir W. Rawlew. Powhatan confessed that hee had bin at the murther of that Colonie: and shewed to Captaine Smith a Musket barrell and a brasse Morter, and certaine peeces of Iron which had bin theirs.

Some time later, after Smith had finished a stay at Powhatan's residence, trading for corn, Sicklemore returned to Jamestown from Chawonock: "but found little hope and lesse certainetie of them were left by Sir Walter Rawley".

Smith then dispatched two others on the same mission, this time with guides from another werowance. This expedition is barely mentioned in the *Proceedings*, but is recounted in some detail in the *Generall Historie*:

> Master Nathanael Powell and Anas Todkill were also by the Quiyoughquohanocks conducted to the Mangoags to search them there: but nothing could they learne but [that] they were all dead.... Three dayes jorney they conducted them through the woods, into a high country towards the Southwest: where they saw here and there a little corne field, by some little spring or smal brooke, but no river they could see: the people in all respects like the rest, except their language: they live most upon rootes, fruites, and wilde beasts; and trade with them towards the sea and the fatter [more fertile] countries for dryed fish and corne, for skins.

Meanwhile, Captain Christopher Newport was on his way back to London, and the London company was going through a reorganization that came very close indeed to destroying the very thing it was designed to revivify, through senseless bungling and a hurricane. However, what is pertinent here is that the reorganized company with its new charter appointed a lord governor to replace the president of the local council, and sent Sir Thomas Gates, one of the patentees of the original charter off to Virginia as deputy governor while his lordship baron De La Warr got himself ready to take over full command. Gates got lengthy and detailed instructions, which included the following paragraph:

> Foure dayes Journey from your forte [Jamestown] Southewards is a towne called Ohonahorn seated where the River of Choanocki devideth it self into three braunches and falleth into the sea of Rawnocke in thirtie five degrees [correctly, 36° N lat.]. This place if you seeke by Indian guides from James forte to Winocke by water, from thence to Manqueocke, some twenty miles from thence to Gaththega, as much and from thence to Oconahoen you shall find a brave and fruiteful seate every way unaccessable by a straunger enemy, much more abundant in Pochon and in the grasse silke called Cour del Cherva and in vines, then any parte of this land knowne unto us. Here we suppose, if you make your principall and cheife seate, you shall doe most safely and Richely because you are in the part of the land inclined to the southe, and two of the best rivers will supply you, besides you are neere to Riche Copper mines of Ritanoc and may passe them by one braunche of this River, and by another Peccarecamicke where you shall finde foure of the englishe alive, left by Sir Walter Rawely which escaped from the slaughter of Powhaton of Roanocke, uppon the first arrivall of our Colonie, and live under the proteccion of a wiroane called Gepanocon, enemy to Powhaton, by whose consent you shall never recover them, one of these were worth much labour, and if you finde them not, yet search into this Countrey it is more probable then towards the north. (Kingsbury, vol. 3: 17).

These instructions were never carried out, for reasons that neither the company nor Sir Thomas Gates could have foreseen. Due to the shipwreck of Gates' flagship, with all three senior officers and all authenticating documents, bedlam broke out in Jamestown when the remaining vesels of his fleet arrived. Smith was accidentally burned and had to return to England, leaving the warring factions there to anarchy, despair, and starvation. When Gates finally arrived nearly ten months later, but sixty of five hundred settlers were alive, and they still had no provisions. Gates was about to abandon the colony when Lord De La Warr providentially arrived, with more colonists, and ample supplies. Even then, however, conditions were far from favorable for any exploring or searching for remnants of Ralegh's "lost colony." Nevertheless, the newly appointed secretary of the council in Jamestown, William Strachey, felt free to confirm what London had written, even though nothing had been done about it:

> This highland is in all likelyhoodes a pleasant Tract, and the Mould fruictfull, especially what may lye to the Southward, where at Peccarecanicke and Ochanahoen, by the Relation of Machumps [brother of one of Powhatan's wives], the People have howses built with stone walls, and one story above another, so taught them by those English who ascaped the slaughter at Roanoak, at what tyme this our Colony under the Conduct of Captain Newport) landed within the Chesapeack Bay, and where the people breed up tame Turkeis about their howses, and take Apes in the Mountayns, and where at Ritanoe, the Weroance Eyanoco [cf. the instructions to Gates, above] preserved 7. of the English alive, fower men, twoo Boyes, and one young Maid, who escaped and fled up the River of Chaonoke, to beat his Copper, of which he hath certayn Mynes at the said Ritanoe, as also at Pannawaick are said to be store of salt stones [rock salt]. (Strachey 1612: 34).

A side note to this passage is even more impressive:

> Peccarecanick between 35. and 36. degrees of the Line: and in her warme Vallyes may be planted, Sugar-Canes, Oranges, Lemons, and all sortes of Southren fruites, and whatsoever, the South-Spaine, Italy, Barbary, Greece, Judea, and Syria bring forth being answerable to the same height.

And just below, he adds, also in the margin: "Likewise Silk of the grass: and habundaunce of the dying Roote."

Although this reads as if something had happened, in reality Strachey probably knew no more than Smith, and no Englishman had in the meantime crossed the watershed between the James River and the Roanoke. Indeed, even when two unhappy colonists tried to run away to the Spaniards who were assumed to live to the south of Jamestown, nothing came of it. Here is what Ralph Hamor, secretary of the colony after Strachey, had to say, referring to such deserters:

> If the law should not have restrained by execution, I see not how the utter subversion and ruine of the Colony should have bin prevented, witnesse . . . . even this summer [1611], Coles and Kitchins Plot, with three more, bending their course towards the Southward, to a Spanish Plantation, reported to be there, who had travelled (it being now a time of peace) some five daies jorney to Ocanahoen, there cut off [stopped] by certaine Indians, hired by us to hunt them home to receive their deserts. (Hamor 1615: 27).

This account was partly reprinted in Smith's Generall Historie in 1624, but apart from this, so far as your speaker knows, from 1611 until 1650 there was no mention of Ocanahowan. The goal was three, four, or five days' journey from Jamestown or some Indian town across the James, yet it was thirty-nine years before any serious attempt was made to find it, and that attempt was accidental, and not intentional, as we shall see. During that interlude several crises came and passed, which must be sketched before we return to the theme.

The kidnapping of Pocahontas in 1613 led to what is called today a détente, that developed into a kind of peace. But both she and her father, Powhatan, died in 1617-1618, after which Opechancanough, heir to Powhatan, began to be restless under continuous English expansion over his lands. As was intimated by Captain John Martin, one of Smith's old companions in Jamestown, and explicitly stated in an explanatory side-note,

> The infinite trade they [the Indians] have had in this 4 years of securitie [1618-1622] enabled Opichankanoe to hire many auxiliaries which in former tymes I knowe for want thereof Pohatan was never able to act the like. (Kingsbury, vol. 3: 707).

The reference is to the massacre that broke the peace on March 22, 1622. The opinion is that of one of the most hard-headed of the colonists, but it tallies well with a few points that are about to come up.

This massacre was followed by twenty-two years of English uncertain expansion and Indian resentment. Then came Opechancanough's second call to rebellion, and the massacre of 1644. After that, the English apparently began to feel that they had the upper hand. A few colonists thought of building up trade with the Indians, for their personal gain. Friendship was not in mind. Neither was Ocanahowan, or Ralegh's lost colonists. It was apparently merely that the English had been settled on the James for nearly half a century, and nobody knew what was lay hidden in the unknown wilderness south of their plantations.

Here we must pause to consider where Ocanahowan in fact was situated; not just where people who had never seen it thought it was. It was not mentioned by the Roanoke colonists, though it must have stood to the west of the Algonquian-speaking tribes whom the colonists visited. We learn from Ralph Lane's expedition, already mentioned, that there were "Countries of great fertilitie adjoyning to the [Chesepians], [such] as the Mandoags, Tripanicks, and Opossians". These were tentatively identified as Iroquoian tribes in Quinn's basic study, but I have been led to suggest that they were all Algonquians. The habitat of the Mandoags and Tripanicks, as stated by Lane, corresponds well with Smith's village names of Mantoughquemend and Teracosick --quite possible distortions--while the third could be an otherwise unrecorded name involving opossums.

But to the west of Lane's explorations lay the land of the Mangoaks, described by him as "another kinde of Savages, dwelling more to the Westwarde" of the Chowan River. This tribe was definitely Iroquoian, although the name by which it was known was Algonquian, and meant "rattlesnakes" or "adders"--a parallel case is the Algonquian name for the Dakotas, from which the English extracted "Sioux," by way of French. While little indeed is known about the Mangoaks, between Lane's comments and the Smith/Zúñiga map it can readily be deduced that Ocahanowan was a Mangoak town, and this is broadly supported by the handwritten notes under discussion, one of which indicates that the local language was different from Powhatan. Again broadly speaking, then, we shall perhaps not be far wrong if we place Ocanahowan west of the Chowan River, and west of the Algonquian tribes of the coastal region, but within the territory occupied by Iroquoians. What then becomes important in the handwitten notes is the date, which can hardly be before 1650. We have heard nothing about the place since 1611.

Interestingly, 1650 was the year in which the first attempt was made to explore and trade in the vast tract of forest and mountains to the southwest of Powhatan village and the falls of the James River. In August of that year, Edward Bland, a merchant who had come to Virginia in 1643, set out with the commandant of Fort Henry, at the falls of the Appomattox twenty air-miles away:

> The little expedition reached an Indian town on the Meherrin river on August 29, having travelled 35 to 45 miles (say 55 to 70 km.). There, at Maharineck, "the great men and inhabitants came, and performed divers Ceremonies and Dancings before us, as they use[d] to do to their great Emperour Apachancano." (Alvord 1912: 119).

This reference to the leader of the two Indian massacres in Virginia is interesting for two reasons: it was made in Meherrin, i.e., Iroquoian, territory, and it seems to show that Powhatan's hegemony extended farther to the south than has commonly been held. (Samuel Purchas' side-note on the Sicklemore expedition, mentioned above, indicates that Powhatan told John Smith something similar.) In any event, on August 30, Bland and his party spoke with a Tuscarora Indian, also Iroquoian, about an Englishman who was supposed to be "amongst those Nations", and learned that the man was "a great way off." Bland then hired the Indian to run on ahead of him and tell the werowance (chieftain) that they "intended to lay him downe a present at Hocomowananck," and wanted him to meet them there."

The Tuscaroras were hardly known at all before John Lawson travelled and lived among them (1700-1709), but they were clearly connected with the Nottoway, Meherrin, and Susquehanna tribes that half-circled Powhatan's domain. Judging by Bland's statement, Hocomowananck was a Tuscarora village. Since Bland spoke with the Tuscarora Indian through a Powhatan-speaking Appamatuck guide (who called the tribe Tuskarood), it is possible that Hocomowananck is identifiable with Ocanahowan (John Lawson's Unanauhan, a Tuscarora village), but this possibility need not be pursued just now. It is enough to know that Ocanahowan was in the territory where one of the Iroquoian nations lived, be it the Mangoak-Nottoways, the Meherrins, or the Tuscaroras.

To carry on with Bland: we read that the Wainoakes (presumably the Weanocks, neighbors of the Appamatucks) attempted to obstruct Bland's traffic with the Tuscaroras, but he went on in spite of them. Crossing the Meherrin River, he travelled perhaps forty miles to the Hocomowananck. On the way, his guides stopped at an Indian commemorative monument in the form of "two remarkeable trees," where the Indians paused and then told Bland that Apachancano had waged war in the neighborhood for "roanoke" (beads, wampum), and for revenge for an insult of the king of Chowan against "one King of a Towne called Powhatan." The commemorative site was kept clean by the friends of either side, and two miles away the grave of a solitary casualty, "a great man of the Chawans," was still regularly covered with green boughs. One can wonder if Powhatan's son Pochins, "king" of Powhatan village, was the one involved. He had been a young man when the English first met him in May 1607.

Not far from there Bland's party at last reached Hocomowananck. They had travelled perhaps eighty miles from Fort Henry, and they had reached another falls. Bland described the place in great detail, adding that not far away Apachancano had appeared one morning with four hundred men and treacherously slain two hundred and forty local Indians. Bland, who was evidently tired of hearing about the deceased "emperor's" exploits, urged his Appamatuck guide to move on: he was eager to trade with the Tuscaroras. But more talk ensued, and more evasion, until Bland finally started back to the fort. To him it was disappointing; yet the record he left is of great value to historians. Hocomowananck was almost certainly at or near modern Roanoke Rapids, North Carolina, about 55 miles (88 km.) south of Petersburg and Fort Henry. It was the extreme southern limit of Powhatan's and Opechancanough's aggression, which had extended his domain north to the independent though related tribes of Delawares in Maryland, Delaware, and New Jersey, and the raiding-trading grounds of the Susquehannas and the half-mysterious Massawomekes, fellow but antagonistic Iroquoians. To the south and southwest of Hocomowananck other nations roamed unmolested by the overlords of tidewater Virginia.

All this fits well into the picture of where the Ocanahowan first mentioned by John Smith must have been. Tentatively we can place it at or near Hocomowananck. Other explorers who followed Edward Bland mention no town which can be taken to refer to the same place, although John Lederer passed through "Kawitziokan, an Indian town upon a branch of the Rorenoke river" early in July 1670, which must have been very near to the path Lederer was following. John Lawson, too, passed nearby a generation later. But after the Tuscaroras executed Lawson in 1711 a general tribal migration started, leaving, of the Iroquoians, only the Nottoway/Mangoaks to people the district around or near Ocanahowan.

It is time now to glance briefly at the other bits of information in the handwritten notes at the back of Robert Johnson's Nova Britannia. Your speaker's formidable ignorance of Algonquian languages generally is relieved only by a superficial and limited acquaintance with the handful of words and phrases noted down by Captain John Smith, and such obvious parallels as he has found in William Strachey's "Brief Dictionary." A few comments may be based on these two sources.

First, as to dating the notes; handwriting is not easy to assign to any given year, or even decade. That under consideration here is too modern for Smith's lifetime, but it lacks characteristics that would stamp it as post-Milton--without implying that it resembles that of either. Let us say, then, that it seems to date from the latter half of the seventeenth century, and that the present speaker leans toward 1650-1660 rather than 1690-1700, though this inclination may be like that of the campanile at Pisa, which stands on a shallow foundation.

Next: several words have been trimmed, making it occasionally difficult to guess soundly what letters are missing. Here a good knowledge of related Algonquian languages would be of help.

Lastly, the details are as follows:

Line 1. Strachey has *keis* for "how many;" Smith has *casa-*. Reconstruct: *case* or *kase*. -- For *riapoke*, Strachey translates "tomorrow." Perhaps some related language can supply the true meaning.

Line 2. The trimmed word *--]up̆*, "tomorrow", requires a specialist.

Line 3. The first word can probably be reconstructed as *nu]ssaconnoke*, "the third day," on the basis of Smith's *nuss*, "3," with *-cunnakack*, "daies," completing the identification. *Maõpunke* is not in Smith, and apparently not in Strachey. A specialist is again needed.

Line 4. The first word should probably be "fou]rth." *Nauńnogh* is not in Smith.

Line 5. The first word, ]unnogh, needs a specialist. Wiuńquot is not in Smith.

Line 6. Here three words need a specialist's attention: ]wãh, "the sixth day;" Ohiãk, "the 9th;" and Teire, "the 10th day."

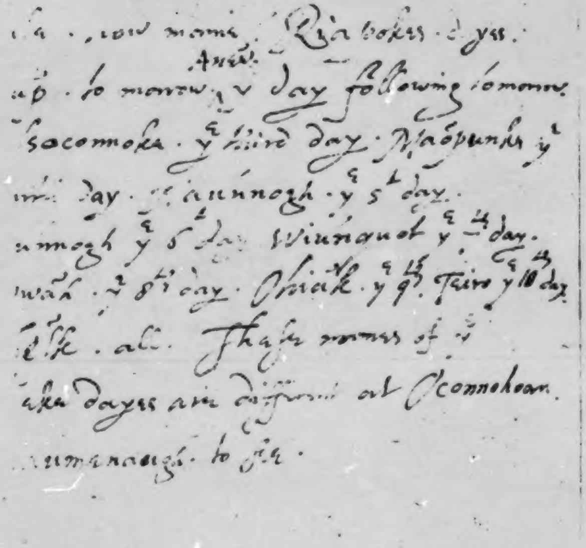
Line 7. Ch]esk can be offered as a reconstruction for "all," since it so appears in Strachey.

Line 8. Both "we]eke" and "w]eke" can be offered as common 17th-century spellings.

Line 9. Strachey has unnamun, "to see" (with a macron over the final "-n", possibly to show that it is not "-u"); hence, suggested reconstruction un]aumenaugh, "to see" (-au- could represent the ah-sound in "father" [not then current in English], or the open-o in "awe, paw"). The word is discussed in Siebert 1975.

The speaker would be interested to know if the linguistic specialists have any suggestions to offer.

To conclude, careful research has lately, and partially accidentally, been producing so much by way of correction of historical details that the speaker is led to hope that similar details in other disciplines can be exchanged all around, so that the jig-saw puzzle which is still the history of the Indian tribes at the time of the European invasion of America can soon be assembled in such form that the students of tomorrow can see the truth of yesterday.



a . how manie . Riapokes . dayes .

Anew

up . to morrow . ye day following tomorrow

saconnoke . ye third day . Maõpunke ye

day . Naunnogh . ye 5t day .

unnogh ye 6t day Wiunquot ye 7t day .

wah . ye 8t day . Ohiak . ye 9t . Teiro ye 10 day

eisk . all . These names of ye

eke dayes are different at Oconnohoan .

umenaugh . to see .

## TRANSCRIPTION

ca ]se . how manie.  Riapokes . dayes

]up̃ . to morrow.  Anẽw. $y^{e}$ day following tomorrow.

nu ]saconnoke . $y^{e}$ third day . Maõpunke $y^{e}$

fou ]rth day . Nauñnogh . $y^{e}$ $5^{t}$ day.

]uñnogh $y^{e}$ $6^{t}$ day.  Wiuñquot $y^{e}$ $7^{t}$ day.

]wãh . $y^{e}$ $8^{th}$ day.  Ohiãk.  $y^{e}$ $9^{th}$.  Teiro $y^{e}$ $10^{th}$ day.

ch ]eĩsk . all.. These names of $y^{e}$

we ]eke dayes are different at Oconnohoan.

un ]aumenaugh.  to see.

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# NOTES ON THE WITTIKO

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## Notes on the Wittiko[1]

The phenomenon of the Wittiko (Cree) or Windigo (Ojibwa) complex among the Northern Algonkians has been one of continuing interest to anthropologists. Recent examinations have included a variety of approaches, including consideration of psychological explanations (S. Parker 1960; Fogelson 1965; Hay 1971), nutritional deficiencies (Rohrl 1970; Brown 1971), ecological and historical causes (Bishop 1973), the role of the Wittiko in "normal" behaviour (Parades 1972), and methodological approaches (Fogelson 1965; McGee 1972) that draw upon the earlier case collection and analysis of Teicher (1960), who viewed the phenomenon as cultural. None of the explanations seem fully satisfactory, however, due in part to the limitations placed upon the investigations by the limited number of cases (the "sample") largely deriving from the nineteenth century, and the limited data contained in the historical literature. A highly promising approach involving the structural analysis of the Wittiko is, however, being developed by McGee (1975) and Fogelson (personal communication). It is my purpose here to add to the literature by providing for the Western Woods Cree the earliest (so far) specific mention of the Wittiko being, an early recorded case of Wittiko "psychosis", an early account of starvation cannibalism, the tradition of the departure of the last Wittiko from the Reindeer Lake region after 1861, and the death of the last Wittiko west of Hudson Bay about 1962[2], and to suggest that Bishop's historical-ecological argument requires modification.

### I

The Wittiko, or non-human cannibal of the Northern Algonkians is depicted as having all or many of the following features: he is of terrifying appearance, with a heart of ice, a skeleton of ice, an enormous giant, he may eat humans, or possess them spiritually so that they themselves become cannibal Wittikos (cf. Teicher 1960:2-5). Other characteristics are sometimes associated, such as whistling or uttering terrifying sounds.

The Wittiko "psychosis" or disorder, which occurred as several types (Fogelson 1965), involves an individual believing that he or she is or is becoming a Wittiko, or the belief of others that he or she is exhibiting characteristic Wittiko and non-human behaviour. In extreme form, the individual actually becomes a cannibal, killing and eating his victims, who are frequently the members of his family, since as McGee (1972:244) notes, "Who else is available?" during the period of winter dispersal when the Wittiko psychosis occurred. The Wittiko and the psychosis are associated with winter, not summer. The behavioural symptoms include melancholy or depression, vomitting or nausea, social withdrawal that includes refusal to eat, to move, or to sleep. The individual who is developing a heart of ice, begins to develop a craving for human flesh, and sometimes sees others as particularly edible animals, such as beaver. Wittiko cannibalism must be differentiated from starvation cannibalism, although it is commonly believed that an individual who eats human flesh acquires a constant craving and thus becomes a Wittiko (Teicher 1960:5-6, passim). Persons exhibiting Wittiko symptoms may be treated in a limited variety of ways and recover, or they may be killed; those who actually become cannibals are killed. Execution is frequently by a member of the family, thus avoiding possible demands for vengeance.

Bishop (1973) has treated the Wittiko within an historical and ecological framework. Briefly, he argues that an aboriginal belief in a cannibalistic non-human person or "supernatural" creature became the basis for the historical Wittiko psychosis in which ordinary humans become cannibals. The Wittiko psychosis, he contends, developed as starvation or its threat became more prevalent as the abundant big game population was catastrophically reduced after the advent of the Europeans and as the full effects of the fur trade were felt. This occurred at different times among the Northern Algonkians: among some Montagnais, near Trois Rivieres and Tadoussac on the St. Lawrence, for example, in the early seventeenth century, and in the late eighteenth to mid-nineteenth centuries among the Ojibwa of Northwestern Ontario (Bishop 1970, 1973) and Cree in the west (Ray 1974:117-24). Thus he explains the predominance of recorded cases from the nineteenth century (Teicher 1960:44), although acknowledging that the limited historical data of the seventeenth and eighteenth centuries may also be a factor to be considered in analysis.

My data pertaining to the Western Woods Cree (i.e., the Cree primarily to the west of Hudson Bay) are pertinent to the question of the aboriginality of the concept, the possible post-contact development of the psychosis, and to the ecological considerations.

To my knowledge, the earliest recording of the word Wittiko was made by Captain James Knight, Governor-in-Chief in the Bay on October 7, 1714. He noted in his entry of that day in York Fort post Journal (Hudson's Bay Company Archives [HBC] B 239/a/1) that:

> Some Indians [Cree] came from Fort Nelson who says thay Saw a Whitego w$^{ch}$ is an Apparition & I keep y$^{m}$ to hunt the Winter.[3]

One may suggest that this perfunctory note reflects Knight's familiarity with the concept, unimportant to him except that it brought him men to hunt during the winter, and that it was not "something new". It is also noteworthy that the Cree were coming from the "bush" to the fort for protection.

In 1743, James Isham (1949:5) reported in a word list the Cree word for "the Devil" as "Whit te co", his knowledge based on his residence in Hudson Bay since the 1730s. Early use of term Wittiko as referring to the devil or Matci-Manitou (in opposition to Kitci-Manitou), a malevolent or cannibal spirit are considered in other sources (Cooper 1934:88ff; Fogelson 1965:75ff).

An early case of classical Wittiko psychosis, in which a man first exhibits symptoms of social withdrawal and then becomes a cannibal amidst abundant game, was recorded in the post journal of Fort Churchill in February 1741 (HBC Archives B 42/a/22):

> February...Down y$^e$ river two Indian [Cree] women, in a most miserable Condition of hunger, one of them is y$^e$ mother of y$^e$ other & they relate y$^e$ following Tragical Story, y$^e$ daughter had a husband & 3 Children, & was one of our goose Hunters Spring & fall Sometime last month this family was in such a Starved Condition that y$^e$ man murthered his youngest Child & Eat it, And in 4 Days after he murthered his Closest Son who was about 12 Years of age, the women fearing he would murder them all they Left him w$^{th}$ y$^e$ dead Boy, taking w$^{th}$ them their Second Child w$^{ch}$ was a girl about 7 or 8 years old, & made for y$^e$ factory, they then being about 150 miles Distance from hence, 3 Days after he pursued them and Coming up w$^{th}$ them he Endeavoured to arrest y$^e$ Girl from y$^e$ mother but both y$^e$ Women Endeavouring to preserve y$^e$ Child, he throatled it in it's mothers hands & after that Seized his wife to Murder her also, but y$^e$ two women over Come him & his wife Knocked him on y$^e$ head w$^{th}$ a hatchet after they had Slue him they Buried him & his Daughter together under y$^e$ Snow & came for y$^e$ factory & in 16 Days time they got here, what is very Surprising at y$^e$ time of this Disaster there was Plenty of Deer [caribou] about them & he had amunition & might have Kild Venison w$^{ch}$ his family Strongly Desired him to do, but he gave no manner of Care to their Sollicitations but his mind seemed to be fixed upon what is above related, y$^e$ Subsistence y$^e$ women got to bring them forward, was for Severall Days Deer Dung w$^{ch}$ they Picked up and Desolved in warm water & So Drank it till at Length providence flung in their way y$^e$ Scraps of a Deer that y$^e$ wolves had Killed & Left....

The location of this event is uncertain. The chronicler places it 150 miles from the fort and presumably in the Churchill drainage system to the southwest. If they had been further south, the women would probably have fled to York Factory. The time was January, which with February is one of the two coldest months of the winter, and the period of maximum social dispersion as necessitated by the distribution of the moose and, especially, the woodland caribou in their winter foraging ranges. In this case, Wittiko symptoms are the cause of starvation, in the midst of plenty, rather than the result. The fort was, again, a place of security and the antithesis of the "bush", as the Wittiko may be the antithesis of human.

Starvation cannibalism was not invariably associated with Wittiko possession as Isham (1949:100-101) reported:

> I Never knew Indians in this part to Eat raw meet, but in case of Necessity, then they do not scruple to Eat one another, to their Greif, or at Least as I'imagine, having Knowne some family's that has Kill'd their Children for food, - Especially one family who was Very much in want of provisions, and being a great way of from the English fort, cou'd gett no Suply (as hungar will Enduce any man to do an unhuman action.) so they made a Dismal Slaughter of four poor Children and Eat the best part of them, which Brought them to the English settlement, in a sorryfull Condition, and of a wild Aspec't, Neither do they come to their Senses and natural way of Living for nigh 3 weeks afterwards, - after such a misfortune when they have Recovd. their right Senses, they commonly take other Indian's Children and bring them up as their own; in Rememberance of the unfortunate Childn. They mascr'd, - their being starv'd an in hungar is oft'd accationed by the Quiqakatches [wolverines]....

The belief was common, at least later, that after a person had eaten human flesh he thereafter developed a constant and insatiable craving for it, and became a Wittiko. Isham's statement indicates this was not invariably so, since the Cree "commonly" adopted children after starvation cannibalism, and that certainly would not have been permitted if the parents thought that adopting parents were incipient Wittikos. Apparently the Cree recognized two varieties of cannibalism, that occurring under starvation conditions, and that resulting from a change to a non-human person, which could occur when game was abundant.

The big game population in the environs of York and Churchill does not appear to have altered appreciably until the mid-nineteenth century, although it seems doubtful if game was ever actually abundant in the Hudson Bay Lowland

(Rogers and Smith 1973:32-3). The major game animal was the woodland caribou (Rangifer tarandus caribou), and reports from 1695 and later indicate that large numbers were encountered; only Drage in 1746 reported scarcity. By 1849 and thereafter, however, they were reported to be scarce (G. Parker 1972:16-18). But while in general caribou were abundant, the post journals and district reports of York and Churchill indicate fluctuations in the caribou population or in caribou territorial distribution. A caveat must be entered at this point. The historical records until the late eighteenth century come from coastal forts, and only occasionally are their direct reports from inland explorers such as Kelsey (1929) in 1689-91, Stewart in 1715-16 (Davies 1965:413-17), Hendry in 1754-5 (Burpee 1907), Hearne (1958) from 1769 to 1772, and Cocking in 1772-3 (Burpee 1908), none of which provide a clear and coherent picture of the local dynamics of the boreal forest ecosystem. Such an understanding must come from ecological studies based on modern conditions (e.g., Shelford 1963; Feit 1973; Tanner 1973), but with the explicit recognition that the big game populations have been depleted since the early nineteenth century, affecting the migratory habits and foraging ranges of the woodland caribou and moose in particular, although doubtless many other species were affected. Yet specific elements contribute to uncertainties connected with subsistence activities: long and short term fluctuations or cyclical variation in faunal populations and the food chain; the budworm, forest fires and subsequent varying seral stages of the forest; sometimes erratic movements or changing foraging ranges of caribou herds; varying severity or moderateness of winters (length, amount of snowfall and depth, blizzards, sustained cold) that affect human and faunal movement; the number of human predators, including the resident population, transients en route to the trading forts; and dislocations of populations due to the fur trade or inter-tribal warfare (e.g., the

Assinboin alliance with the Cree-Ojibwa).

## II

One obstacle to the understanding of the Wittiko psychosis has been the limitation of data, since the anthropologist did not appear on the scene until after the occurrence of actual cases. It seems pertinent to record the Cree explanation for the disappearance and death of the Wittiko.

The Rocky Cree (Smith 1975) occupy the region north and west of the Nelson River, in northern Manitoba and Saskatchewan. They are bounded on the east and south by the Maskegon or Swampy Cree, on the west by the Strongwoods or Bois Fort or Thickwoods Cree, and on the north by the Athapaskan speaking Chipewyan. The Cree around Reindeer Lake are situated in the northern transitional zone of the boreal forest (Row 1972), and subsistence was based on moose and the southern extension of the barren-ground caribou (R.t. groenlandicus) herds which are replaced to the south by the woodland variety. Reindeer Lake was aboriginally in Cree territory, largely abandoned to Chipewyan exploitation in the nineteenth century (perhaps partially as the aftermath of the great smallpox epidemic of 1781), and in the nineteenth and twentieth centuries re-occupied by Cree returning from areas further south. The return was initially as Home Guard Indians regularly or seasonally employed by the Hudson's Bay Company.

The Cree recognize the Wittiko complex. They conceive of the Wittiko as of terrifying appearance (although there is no consensus on details), average size but with a heart of ice, enormous strength, the ability to paralyze humans, and a cannibal who also had the power to transform mortals into cannibals. On the question of the Wittiko's stature, I noted to informants that among Cree and Ojibwa to the south the Wittiko was a giant. They maintained, however, that the Wittikos of this area were of average size although they accepted the giantism in the south. They noted that many northern creatures were smaller in "the land of the little sticks": the barren-ground caribou are smaller than the woodland variety, there are fewer big beaver, etc.

The last Wittiko they assert left the Reindeer Lake region after the establishment of the mission, as his power was not as great as that of the church. He was apparently still active to the south, but why Christianity was less powerful there is not explained. (One may note the history of European intrusion in this area. A North West Company post was established at the south end of the lake about 1790; the HBC established Bedford House in 1794-5, and subsequently there were other short term operations. In 1859 the post at Brochet was established, and was joined in 1861 by the Mission St. Pierre de Lac Caribou of the Oblates of Mary Immaculate. After a network of trading post-mission complexes were established the threat of starvation was greatly reduced, and, perhaps, the Christian devil became the substitute for the Wittiko.)

The last Wittiko was reported to have been killed about 1962. The Wittiko was extremely hungry, having been deprived of human flesh for a long time. In desperation, he decided to stop the Canadian National Railroad train on the run from The Pas to Churchill and to eat the passengers. He stood on the tracks before the approaching train and attempted to paralyze the locomotive engineer and crew, and to stop the train, by his terrifying appearance and superhuman power. His power was inadequate, and he was run over and killed. There have been no Wittikos reported since.

If one considers the stories of the departure and death of the Wittiko as a metaphor, they have interesting ecological implications. The power of the Wittiko was not as great as that of the Church, but the establishment of the mission followed closely on the network of trading posts that could provide emergency food. One notes also that earlier Cree had come to Churchill for protection against the Wittiko - and Churchill was a source of food in emergencies. The last Wittiko was killed by a train, and I must note that the line was built to open Churchill as a port: the main export from the south is grain, and the train supplies the food and other commodities for the town as well as the HBC posts on the line. Modern technology and the extension of stores and social services in this century has eliminated the threat of starvation except in most unusual circumstances, and the Wittiko is dead, killed by European civilization. In a broader ecological sense, past generations saw an increase in the number of HBC posts and the development of villages leading to, perhaps, a greater psychic or social security. Still, time is spent in the insecurity of the bush, and the legend of the fearsome Wittiko remains, even among the very young.

## III

The Wittiko complex has been a characteristic of the Northern Algonkians at least since the early seventeenth century. Teicher (1960:17), and others, on the basis of the few recorded seventeenth century cases, argued that it was aboriginal, while Bishop (1973) considers the same cases to be the historical beginning of the Wittiko psychosis in the context of a declining big game population. He considers the rise in the number of reported cases of the psychosis -- four in the seventeenth century, five in the eighteenth, thirty-six in the nineteenth, fifty of unknown date (Teicher 1960:44) -- to be indicative of a reaction accompanying greater probability of starvation, or of its threat. Of the seventy cases recorded by Teicher, only nine refer specifically to the Cree (forty-five to the Ojibwa-Saulteaux, and eleven to the Montagnais-Naskapi), and to these Bishop (1973:10-12) adds several cases from Forts Albany and Severn, and this writer data from York and Churchill.

Bishop believes the Fort Albany (on James Bay) famines and starvations in 1706 and 1707 are part of the process of localized game depletion of a nature to give rise to starvation or Wittiko cannibalism. On the other hand, G. Parker's (1972:16-19) data do not indicate a general depletion of the caribou to the west of Hudson Bay. My interpretation is that we may perceive equally well either fluctuations in herd movements and distributions, and/or cyclical fluctuation in herd population - both common characteristics of most animal species. In other words, the ecological dynamics of the aboriginal Subarctic environment west of Hudson Bay offered the necessary preconditions for the entire Wittiko complex of cannibal giant and psychosis.

Bishop (1973:13-14) writes that "the early literature for the Ojibwa strongly suggests that had institutionalized patterns whereby cannibalistic impulses could be directed toward non-Ojibwa", and that they "appear to have had well developed techniques of social control within groups". He then cites Dollier and Galinee near Sault St. Marie to show the abundance of moose there in 1669-70, and Perrot who noted 2,400 moose were killed on Manitoulin Island in 1670-71. Thus, he argues, stronger social organization and abundant game inhibited the development of the Wittiko psychosis until game depletion subsequently occurred.

I should like to make several comments here: first, the early literature of the seventeenth century is limited, highly variable, and indicates that many Algonkian groups ate enemies killed in battle, but endo-anthropophagy only in case of starvation (Perrot in Blair 1911, I:103, passim). To refer to institutionalized exo-anthropophagy for the Ojibwa-Cree specifically may be an overstatement. Similarly reference to "well developed mechanisms" of in-group controls may overly stress what is only at best inferred from Perrot, the Jesuits, and other early writers. One may grant, however, that among some proto-historic Northern Algonkian groups social organization may have been somewhat more elaborated than later, when the HBC traders assumed great economi and political power, and were followed by missionaries and government officials Secondly, while moose may have been abundant near Sault Ste. Marie in 1669 and on Manitoulin Island in the winter of 1670-71, there were substantial or extrem local variations, as Father Louys André's (JR 55:133-55) account of 1670-71 indicates. He left Sault Ste. Marie in the summer of 1670, and arrived among the Missisauge (Missisauga) who were on the banks of a river rich in sturgeon, but "all these people had for some time been suffering from a famine, and I found them reduced to a fir tree diet" (pp. 135-7). Then among the Beaver Nation (Amikwa) there must have been abundance, for some 1500-1600 Indians of many nations were assembled for a festival involving the eldest son of a deceased chief taking his name (pp. 137-38).[6] On Manitoulin Island he almost died of hunger, for the Ottawa had "ill success in fishing and hunting" (pp. 143-45), and the "continuance and increasing severity of the famine and dispersing all the people" forced him to close the mission (p. 147). He then went to the Nipissing for three months, where they suffered greatly from hunger, and kept alive on acorn and rock tripe, a lichen (pp. 147-53). After the melting of the ice, he returned to the Beaver, where there were enough moose to live in "comparative comfort" (pp. 153-55). Thus in an early Ojibwa historical context and within the limited area north of Georgian Bay and before the catastrophic big game depletion, there was great variation in the local and presumably seasonal availability of big game and/or fish. Again, the micro-dynamics of the Subarctic ecology were suitable for the emergence of the Wittiko complex.

The curious "tribal" distribution of Wittiko psychosis has not received adequate attention. Teicher's (1960:44) figures show that forty-six or 70% of the sixty-seven cases that can be assigned to tribal groups occurred among the Ojibwa (including Saulteaux and Beaver), and only 30% of all other Northern Algonkians (Cree, Montagnais-Naskapi, Tête de Boule). The "new" case from Churchill and two cases added by Bishop for Fort Albany increase the incidence of occurrences among the Cree from nine to twelve, or by a third. An environmental explanation for the disproportionate number of Ojibwa cases reported may be that faunal catastrophism was greater in this region than elsewhere, as Bishop (1974:183-227) has documented the virtual annihilation of big game in the Ojibwa area between Hudson Bay and Lake Superior. While game depletion occurred through most of the Western boreal forest, Ray (1974:122-23) indicates relatively abundant game in the southwest, and I have the general impression that big game reduction was not as severe west of Lake Winnipeg and Hudson Bay. On the other hand, the conditions of the sample must be critically reviewed. A European had to be present at the right time and place, and under the right conditions, to learn of and make a record of the incident, and it appears that there were more trading posts in the Ojibwa territories than elsewhere (Voorhis 1930, map). For a case to be recorded in the HBC records for example, an Indian trading chief had to think it of some interest to the trader and to mention it; the trader had to think of it as of some consequence to the Company to record it. Early traders, missionaries, and explorers often tended to stress the exotic, while others were uninterested in and perfunctorily dismiss native "superstitions". As early as the 1790s, David Thompson (1916:82) noted the reticence of the western Cree to discuss their religious beliefs, perhaps as a result of the ridicule to which they were probably subjected. Adding to the unreliability of the statistics is the tendency of some writers to conceal or understate cannibalistic events, including H.R. Schoolcraft (who was married to an Ojibwa), and acculturated, literate Ojibwa such as Warren, Jones, and Copway.

However, among the Ojibwa, many Northern Algonkian socio-cultural phenomena seem more emphasized or intensified. These features include "atomistic" or fissionable social organization, witchcraft and sorcery, the potential malevolent power of the higher degrees of the Midewiwin or Grand Medicine Society, suspicion of others, etc. The Nipissing, one of the bands or "Nations" that in later history are part of the Ojibwa, were known to Champlain and the early Jesuits as the "Nation of Sorcerers". In the 1790s David Thompson (1916:246) observed that "Of all the Natives, these people [the Ojibwa] are the most superstitious, they may be accounted the religionists of the North."

In attempting to explain the historical and ecological background of the Northern Algonkian Wittiko complex, the early references to the Wittiko and the "Apparition" suggest that it is aboriginal when taken together with the micro-dynamics of the boreal forest, and that Wittiko psychosis may equally have occurred. Under the environmental stresses of the late eighteenth and early nineteenth centuries, cultural drift (Eggan 1963) may have given greater direction, noticeably stronger perhaps among the Ojibwa than among other Northern Algonkians.

McGee (1972:245) has questioned the absence of the Wittiko phenomenon among the adjacent Northern Athapaskans, but the entire complex must be considered: it is not sufficient to note that starvation cannibalism occurred, that starvation anxiety finds expression in oral or that legendary giants sometimes eat people (e.g., Jack and the Beanstalk). The complete complex involves a non-human cannibal (although somewhat anthropomorphic) and associated characteristics, and the belief that humans may become Wittiko cannibals, marked by asocial symptoms of withdrawal, and actual Wittiko cannibalism. As a complex, it does not appear indiginous to the Northeastern Athapaskans. The myth of the "Windigo" was recorded by Lowie (1925:325-36) for the Chipewyan, based on his very brief trip to Fort Chipewyan (Lowie 1909), but cannot be accepted as a part of the "genuine" oral tradition. Goddard (1912:53-4) also records a cannibal monster for the Chipewyan. Alexander Mackenzie (1927:131), on the basis of his years of explorations, wrote of the Chipewyan:

> If there by any people who, from the barren state of their country, might be supposed to be cannibals by nature, these people, from their difficulty they, at times, experience in procuring food, might be liable to that imputation. But, in all my knowledge of them, I never was acquainted with one instance of that disposition; nor among all the native which I met with in a route of five thousand miles, did I see or hear of an example of cannibalism, but such as arose from that irresistible necessity, which has been known to impel even the most civilized people to eat each other.

I have worked with the Barren Lands Band of Chipewyan, who have known the Cree for more than a century as the "Home Guard" of the HBC post at Brochet. The Chipewyan know the story of the Wittiko from the Cree, and accept that there were once Wittikos in their [Cree] area. They do not, however, have any tradition of having ever seen one or having had a case of the psychosis or related phenomenon. Indeed, oral traditions give little part to starvation, although often enough mentioning frequently periods of great hunger. They do, however, accept or respect the beliefs of their neighbours much, I suppose, as an ordinary resident of Rome in the Imperial Age respected the Egyptian cult of Osiris.

To the south of the Ojibwa, however, Landes (1968:14, 59-63) reported the occurrence of starvation cannibalism, shamanistic cannibalism, and abnormal cannibalism among the Mdewakantonwan Santee Dakota of the Woodlands.[7]

Starvation cannibalism may be attributed both to the reduction of game population or to severe environmental conditions, such as blizzards. Shamans and their Supernaturals were considered ritually cannibalistic. Among the Santee there were stories of cannibals who were considered shamans, including one of brothers who were both cannibals. Landes (p. 61, fn.) suggests that Ojibwa as one source of Santee belief, and the intermarriage of Ojibwa and Dakota during the period of peaceful coexistence in the late seventeenth and early eighteenth centuries (Perrot, in Blair 1911, I:277) may have provided the mechanism for diffusion.

More importantly, however, the cannibal giant or Wittiko is not restricted to the Subarctic Algonkians. It occurs with variations in the mythology of other Eastern Algonkians: among the Micmac he is known as Chenoo, to the Malecites, Passamoquoddies, and Penobscot as 'Kiwaqu', and in Western Abenaki as Kiwakwa, the "forest wanderer". Among non-Algonkian people variants the Huron Strenda and Iroquoian Stonecoats or Stoneclads, and the Cherokee equivalent, its heart of quartz, are equivalents. The distribution of a cannibal-giant complex over such a wide geographical area including hunting, foraging and horticultural adaptations (as well as different language families) suggests that something more than a direct cultural ecological relationship is involved and that significant time depth was present.

## IV

The Wittiko cannibal giant and the psychosis have long been recognized as having an ecological component reflecting the hardships of Subarctic winter existence. Bishop has carried the environmental interpretation to an extreme form, arguing that the Wittiko complex, stemming from an aboriginal generalized cannibal giant, attained its specific historical features as the consequence of catastrophic megafaunal population destruction in the post-contact period.

Considered only within the Northern Algonkian region, a number of factors tend to support the thesis that the Wittiko complex was aboriginal. Boreal forest dynamics alone provide conditions under which substantial areas would periodically have inadequate big game populations. The early Jesuit report, cited above, provides historical evidence for local Ojibwa food shortages prior to intensive European contact. The distribution of the Wittiko cannibal giant and the psychosis, west of Hudson Bay in the earliest historical period and prior to any significant environmental damage, also suggests that aboriginal conditions accounted for the phenomena. Starvation cannibalism sometimes occurred among the Western Woods Cree, but was not necessarily associated with the transformation of the unfortunate anthropophogist into a Wittiko. On the contrary, it was possible for the transformation to occur in the midst of abundant game, suggesting that factors other than threat of starvation were involved. Finally the distribution of recorded cases of human Wittiko cannibalism at least partially suggest that the statistics are limited by the accidental presence of a European to record the cases. The high incidence of cases among the Ojibwa is to be partly explained in this way, although the phenomenon "fits" with the extreme development in other related areas of their culture.

If the Wittiko complex is considered in the more general context of the Eastern Woodlands, it is but one specific manifestation of a very basic cultural theme in which ecological factors have only a part. The very widespread Wittiko concept cannot be accounted for by post-contact diffusion. The acceptance of the Wittiko myth by the Chipewyan, relative newcomers to their area, presumably indicates that the myth has significant meaning on a cross-cultural level. As McGee and Fogelson suggest, it seems to be a cultural phenomenon that functions to define the essence of humanness.

## Notes

[1] The Cree term is used here, rather than the Ojibwa since the data presented here refer to and are derived from that group. The quotations from the Hudson's Bay Company archives are published with the permission of the Hudson's Bay Company, which is gratefully acknowledged.

[2] H.F. McGee informed me that a Wittiko "is alive and well in the Ungava".

[3] Published by permission of the Hudson's Bay Company.

[4] Published by permission of the Hudson's Bay Company.

[5] The word ogema is translated by the Southwestern Ojibwa as chief; among the Northern Ojibwa ogema (or Cree okima) is translated as "boss" and refers to the HBC manager, and okima·hka·n or "boss-surrogate", is applied to the elective chief (Ellis 1960:1).

[6] Perrot (Blair 1911, I:221) reported that 2,400 moose were killed in winter of 1668-69 on Manitoulin Island, but his editors, Tailhan and Blair (Ibid., p. 212, fn. 158), give the years as 1670-71 on the basis of the time when the Sieur de Saint Lusson, whom Perrot accompanied, took formal possession of the territories in the name of France (JR 58:105 ff). Father Andre's report, cited here, indicates that in the winter of 1670-71 famine conditions prevailed in several band territories. One is tempted to suggest that the moose were exterminated by the Beaver Nation for their great feast, even as the Ottawa were returning to the island (JR 55:133) after having fled from the Iroquois.

[7] I am indebted to Ruth Landes for pointing out the Santee data to me.

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# ETHNO-HISTORY AND ARCHAEOLOGY OF THE MUSHUAU INNUTS

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## INTRODUCTION

The Mushuau Innuts were a group of Indians that inhabited the region around Indian House Lake (Fig. 1). The lake (54X2 miles) is the widening of the George River and is located 155 miles northeast of Schefferville, 150 miles south of Ungava bay and 120 miles west of the coast of Labrador. The Mushuau Innuts were often referred to as the Barren Ground band or George River band (Speck, 1931). Many authors have referred to them as the true Naskapi (Cabot, 1912; Hubbard, 1908). Neighbouring Indian groups were the Fort Chimo, Davis Inlet and Michikamau bands (Fig. 2). The term Mushuau Shibo Ilnuts was at least used by Michikamau Indians to refer to the George River group (Tanner 1944: 590). In Montagnais dialect, the word means Indians of the river without trees. The meaning does not describe truly the forest-tundra vegetative conditions since today many trees cover the lowlands. Dendrochronology also supports the existence of trees during the occupation of the lake from 1850 to 1916.

Since 1973, we have been carrying out archaeological research at Indian House Lake. Research was funded by the Service d'Archéologie et d'Ethnologie, Ministère des Affaires Culturelles, Québec. Initial surveys of the lake in 1973 and 1974 revealed the presence of 44 historic Indian camp sites. The sites yielded a total of 681 habitation infrastructures. Most of these date from mid 19th century to early 20th century and are attributed to northern Algonkian populations. Last summer, excavations were initiated at a large site (HEDF-4) containing 105 habitation infrastructures. Five dwellings and one exterior structure were completely excavated.

We wish to present here the preliminary results and interpretations of the archaeological work but first we wish to present an ethnohistoric synthesis of the Mushau Innuts.

## ETHNOHISTORIC SYNTHESIS OF THE MUSHUAU INNUTS

Unfortunately, we have only sketchy glimpses of the Mushuau Innuts during the 1850 - 1916 A.D. period. This period is referred to as the Barren Ground period (Tanner, 1944). In the early 19th century Erlandson and J. MC Lean were among the first white men to penetrate the George river. It is only with the travels of Mina Hubbard and William Cabot at the beginning of the 20th century that more direct and vivid accounts were given. Later ethnographic studies were carried out on the same population which had departed Indian House Lake (I.H.L.) for Davis Inlet in 1916 and had modified its subsistence-settlement pattern (Strong, 1929; Waugh, 1925; Tanner 1944; Henriksen 1973.) From 1916 onward some Indians continued for a time to exploit the I.H.L. resources on a seasonal basis while others became involved in trapping fox closer to the coast. Today I.H.L. is part of a fishing and hunting reserve administered by the provincial government.

The last glimpses of the Mushuau Innuts in their hearthland come from Hubbard (1908) and Cabot (1912, 1920). They give us a vivid account of their life style from 1903 to 1910. We can witness a population already somewhat acculturated and engaged in regular transactions with the Davis Inlet Hudson Bay post. The Mushuau Innuts made regular trips to the post each summer around the beginning of August. There they exchanged their few furs for necessities such as tobacco, tea, powder, kettles, spy glasses, blankets, hooks, line, nails, harmonicas, handkerchiefs, eskimo boots, leggings, and pork.

The pictures taken by the travellers are not only useful in determining the degree of acculturation reflected in material culture has also in locating old camp sites. For examples, from a 1910 photograph (Fig. 3) taken by Cabot, we were able to discover the Tshinutivish camp site, witness the number of tents, their distribution and type of habitation. The travellers' accounts also describe lookout stations (Fig. 4), communal caribou hunting methods from canoes, food and hide preparation by women (Fig. 5), food storage (Fig. 6), identify members of the same band over a 7-years period, give demographic estimates and number of tents at campsites. Furthermore, in 1905, corroborating statements were made by both Cabot and Hubbard concerning the location of Mushuau Innut 1905 summer camp (Fig. 7). They also confirm the presence of the same Indians with chief Ostinitsu.

Prior to 1903, information on the Mushuau Innuts at Indian House Lake is practically absent. Therefore, we are left to speculate as to when the Mushuau Innuts first arrived in the region. The establishment of Fort Trial or Erlandson on Indian House Lake from 1839 to 1842 was perhaps motivated by the presence of Indians in that area. The site of the post was said to have been near a traditional gathering site. In 1974, efforts were undertaken to locate the H.B.C. post. Only the Thsinutivish (Fig. 3) or 1910 summer campsite was located near the presumed post. Eight infrastructures were present at $H_aD_e$-15: one rectangular and the others more or less circular. Excavations are planned for 1976.

Numerous hypotheses on the origins of the Mushuau Innuts have been presented. It was suggested that demographic presures and decline in caribou herds further south (Fitzhugh, 1972, Rogers and Updike, 1968) or forest fires (Tanner, 1944) forced a northward movement of Indians into the forest-tundra. Some proposed the idea of a gradual northward shift on the George River accompanied by increasing acculturation and demographic decline (Rousseau, 1948). For the moment these hypotheses remain unconfirmed. Many maps exist showing the distribution of Montagnais-Naskapi groups at different times but their interpretation is difficult. Our preliminary archaeological results permit only timid and cautious statements on that particular topic.

## ARCHAEOLOGICAL RESULTS ON THE HISTORIC MUSHUAU INNUTS

The 45 historic sites attributed to the Mushuau Innuts are evenly distributed along the lake but tend to occur at the constricted sections, at the mouth of tributaries and in the proximity of eskers (Fig. 8). Sites consist mostly of clusters of tent infrastructures and are located at elevations between 5 and 15 meters above lake level. A few burials, caches and lookout structures were also found. The number of tent infrastructures at each site varies generally from 7 to 105. No systematic surveys in different micro-habitats have yet been attempted in order to increase the representative value of the sample on subsistence-settlement patterns.

A tent infrastructure consists of a more or less round earthring with a central hearth and an occasional ramp from the entrance to the central hearth (Fig. 9). Diameter generally varies from 3 to 4 meters for the circular-truncated type while shaputuans vary from 5 to 7 meters long by 3.5 to 4 meters wide. The largest Shaputuan (Fig. 10) with 3 hearths measured 9.40 meters long by 3.95 meters wide. The interior of most earthrings is depressed to depths of about 10-15 CM. To a practised eye the infrastructures are very conspicuous on the exposed terraces covered only by moss, lichens and shrubs. Often due to differential wind exposure, infrastructures appear as crowns of vegetation. Such archaeological features may be detected on black and white aerial photos at a scale of 1:15,000.

Two types of earthrings are easily distinguished: 1) circular-truncated and 2) rectangular with rounded ends. The first type of tent infrastructure corresponds to the conical suprastructure of wooden poles about 18 feet high called Tshistwehigantshop (Fig. 11). Two or three strips of caribou hides usually covered the frame. The second type corresponds to a suprastructure with 2 A-frame and a ridge pole against which smaller poles were laid (Fig. 11). This last structure has two central hearths and two entrances at each end. The term Shaputuan is usually associated with such a suprastructure except that normally there are 3 hearths instead of 2. Only one case of a 3 hearth Shaputuan was located at HeDf-4.

Such tent earthrings were also reported from the Koksoak River (Lee, 1967) and from Great Whale River (Harp, 1968). Thus, these features seem to have predominated among Fort Chimo, Great Whale River and George River Indians. Their southern occurence is unknown but their suprastructural counterpart was widespread all along the north coast of the St. Lawrence River.

Many questions arise from the archaeological data that we will examine here. First, what is the social unit to be associated with tent clusters? Secondly, how closely can we evaluate the demographic representation of each site, of each tent ring? And finally, how are the building techniques of tent suprastructures reflected in dwelling infrastructures?

## TENT CLUSTERS AND SOCIAL UNIT

A certain number of tent earthrings seem to reoccur in clusters at different sites. We hypothesize that each cluster of 4-7 earthrings represents a synchronous occupation by a local band attached to a particular drainage basin such as the Mushuau Innuts to the Mushuau Nipi. The hypothesis is based on the reoccurence of such clusters containing about the same number of earthrings, and the general reoccurence of one Shaputuan in each cluster. The spatial distribution of tents and orientation of the entrance in each cluster is also suggestive (Fig. 12).

Dating is important to determine synchroneity of occupation. With dendrochronology each infrastructure will hopefully be dated. At the moment, no discernible chronological distinctions can be realized with the use of the European material surface collected at the sites. Most of the European material consists of cartridges (44-40, 30-30, 44, 38), metal file fragments, metal awls, tin cans, minuscule glass beads, tobacco stamps, nails, glass, pieces of canvas, one 1894 penny with the effigies of the Queen of Victoria and the province of New Foundland. Percussion caps are among the only indicators of earlier occupation.

The ethnographic data suggests that a Mushuau Innut summer camp site in the early 20th century consisted of about 2-4 tents with as many as 60 individuals. The archaeological data revealed an average of 4-7 tents per occupation. The greater number of tents in comparison with ethnographic data may be explained either by the practise of moving lodges around for cleanliness (Leacock, pers. comm.) or by a larger population. We tend to favor the former explanation. Sub-clusters could be possible along socio-economic ties.

The presence of one Shaputuan per cluster of 4-10 tents may be interpreted as: 1. the chief's household, 2. a feasting or ceremonial hall, 3. a multi-family household, 4. a combination of no. 1 and 2, and 5. a combination of no. 2 and 3. We favor the latter interpretation with the reservation that habitation had multifunctional uses. On the basis of ethnographic data, we doubt that the Shaputuan was the chief's residence. In fact, Wallace (1907: 135) informs us that in a camp consisting of one oblong tent with 3 fireplaces (Shaputuan) and one round (Circular-truncated) wigwam, chief Toma occupied with his family the smaller lodge while the others made their home in the larger one. This camp is located on the upper George River and is possibly the same one visited by Mrs. Hubbard on Resolution Lake (Fig. 13).

The circular-truncated tent rings likely represent individual household units while the Shaputuan could house two or more families and may be the scene of other socio-economic activities such as the Mukushan. Local bands according to ethnographic data (Michaud and Mailhot, 1965) were made up of a more permanent core personnel and a mobile personnel of Indians shifting from band to band according to personal and socio-economic desires.

Larger archaeological sites such as HeDf-4 and HcDe-1 with 105 and 54 tent earthrings respectively are tentatively interpreted as regional gathering sites of two or more local bands. The ethnographic literature (Graburn, 1969) speaks of I.H.L. as the gathering place of Fort Chimo and Davis Inlet bands. These sites were most likely reoccupied seasonally for caribou hunting. These meetings gave also rise to various social and economic exchanges. This last social unit may be seen as the largest endogamous group in the area. Such camps may have been represented by as many 20-30 tents. Other hypotheses may be given for the sites: 1. 2 or more diachronous occupations by 2 or more local bands and 2. reoccupation of the same site by a local band over a great many years. Excavations should provide answers to this question. Dendrochronology and study of refuse deposits should permit us to establish the diachroneity of occupation. From the faunal material we should also be able to distinguish between spring gathering and fall-winter gathering campsites.

We are confident that by and large tent clusters represent synchronous occupations since during surveys and excavations we came upon very few cases of possible reoccupations of the same infrastructures. Also surveys produced no cases of overlapping earthrings. Nevertheless, it remains open to question whether or not reoccupation of the same infrastructure in succeeding years after initial occupation would register archaeologically. Other types of sites could also be identified from the archaeological remains. We should be able to distinguish specialized camps (2-3 tents) such as lookout stations, winter disbandment camps and travel camps.

## DEMOGRAPHIC ESTIMATE OF CAMPSITES

A corrolary question to the study of social units represented by tent clusters is the demographic component of each campsite. Pertinent information is supplied by the ethnographic literature of I.H.L. In fact, Hubbard and Cabot state that campsites consisted of 2-3 tents. Later demographic estimates (Waugh, 1925; Tanner, 1944; Speck, 1931) vary between 36 and 200 individuals. Fifteen to twenty occupants per tent appear as a high figure in comparison with other ethnographic literature. A possible explanation may come from the larger size of the tents. Cabot (1912) describes one lodge at Tshinutivish as large as nearly 50 poles and housing 7-8 persons, but it could have contained 20 individuals. Furthermore, at Tshinutivish only 7 infrastructures were found despite its being occupied in 1903 and 1910. Using the average of 4 tents for a population of 40-60 individuals, one arrives at an acceptable figure of 10-15 occupants per tent.

At HeDf-4, a detailed analysis of surface area inside tent earthrings (Lobe -1, Terrace -2) gave a mean of 138,474 $cm^2$. Taking into account the surface area of the central hearth (about 7,850 $cm^2$) 130,624 $cm^2$ remain for occupation. Estimating a minimal surface area of 20,000 $cm^2$ per occupant we arrive at an average of 6.5 occupants per tent. Archaeological estimates are at odds with the ethnographic data. Our computation methods are most probably faulty because at least 10-15 occupants could live in the tents. Experimentation will eventually help resolve the problem.

## BUILDING TECHNIQUES AND DWELLING INFRASTRUCTURES

The final question of interest concerns the technological aspect of domestic habitations. We wish to study the correlation between technological elements and the archaeological remains. Particularly, we wish to find out how architectural aspects are reflected in the archaeological infrastructure.

For this purpose, a number of infrastructural attributes were devised and studied during surveys and excavations. As an example, we may discuss the case of the circular-truncated infrastructure and its attributes (earthring, truncation, depression, hearth, etc.) (Fig. 9). First, the earthring is the result of interior excavation before building the conical suprastructure. The sand, gravel and stones were removed from the interior and scraped toward the periphery. They became part of the earthring. They also gave additional strength and protection to the structure, and served as pillow for sleeping. Turner (1894) also mentions that the conical suprastructure was 10-14 feet high and 10-18 feet in diameter. The latter figure agrees well with the data at HeDf-4 presented earlier. Size would have varied with the number of occupants.

The truncation on the entrance side is tentatively interpreted as the result of 3-pole foundation instead of a 4-pole foundation as reported among the Cree of Mistassini. The 4-pole foundation would normally produce a squarish infrastructure. Experimentation will be carried out in 1976 on this question. In addition, we stress the importance of detecting post molds, coniferous needles and twigs and animal hairs. During excavations we recorded the presence of numerous twigs on the surface and also under the peat. We hypothesize that these twigs are the remains of spruce boughs placed by the Indians inside the tent.

We believe that there is a correspondance between architectural elements and the archaeological attributes. Using such an approach we may distinguish other structures such as dome-shaped tents, sweat tents, shaking tents, caches and lookout stations, Shaputuans, (36 X 12 feet), Wishketsanshop (20 X 8 feet). Differences in structural elements between the latter two are the number of central hearths and the number of entrances. In the other cases, a series of test propositions are deduced from the ethnographic descriptions and verified through excavations. For example, the sweat tent would lack charcoal, bones and possibly an earthring. The presence of fire cracked stone and small size would be further evidence of its function, but multi-functional uses should always be investigated. Wallace (1907: 125) describes sweat holes among Labrador Indians as about "2 feet in depth, large enough in circumference for a man to sit in the center and surrounded by a circle of good-sized boulders. Small saplings are bent to form a dome-shaped frame for the top" (Fig. 14). Such sweat holes were possibly located at HeDf-4 (Fig. 15) but stones and the sapling suprastructure were missing.

## CONCLUSION

Many of the above interpretations will receive a firmer status when on-going analyses are completed. Our contention is that archaeology at I.H.L. combined with ethnographic studies at Davis Inlet will eventually piece together the fairly unknown ethnohistory of the Mushuau Innuts. Such studies are being contemplated for 1976 and these could benefit from present archaeological maps, photographs, artifacts and other relevant data.

No definite answers can be given. We believe more and more that concerning the origins of the Mushuau Innuts a post - 19th century arrival at I.H.L. appears likely. On the other hand, there is no evidence supporting a gradual shift northward along the lake itself. On the contrary, many early sites lie in the northern section.

Our archaeological research will proceed in the coming years to test the various models or hypotheses put forward on habitation, demography and social organization.

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Fig. 8: Distribution of Historic Algonkian sites. *Mushuau Nipi*, Nouveau-Québec.

Fig. 9: Typical circular-truncated earthring with archaeological attributes.

Fig. 10: Infrastructure with 3 hearths and earthring outlined by chalk. Interpreted as a ridge pole suprastructure with 2 entrances. (*Shaputuan*). Site HeDf-4, Terrace-3 structure No. 82, 1973.

Fig. 11: Conical and ridge pole suprastructure. Montagnais camp at Montagnais Point, Lake Resolution (1905).

Fig. 12: Distribution of tent earthrings at HdDe-2, *Mushuau Nipi*, Nouveau-Québec.

Fig. 13: Montagnais camp on the Upper George River.

Fig. 14: Abandoned suprastructures: sweat tent (*Nitshentshop*) and conical tent (*Tshistwehiganthop*), Lake Michikamats (1905).

Fig. 15: Circular depression interpreted as a sweat hole. Diameter: 2.25 meters; Depth: 50 cm. Structure No. 92, HeDf-4, *Mushuau Nipi*, 1973.

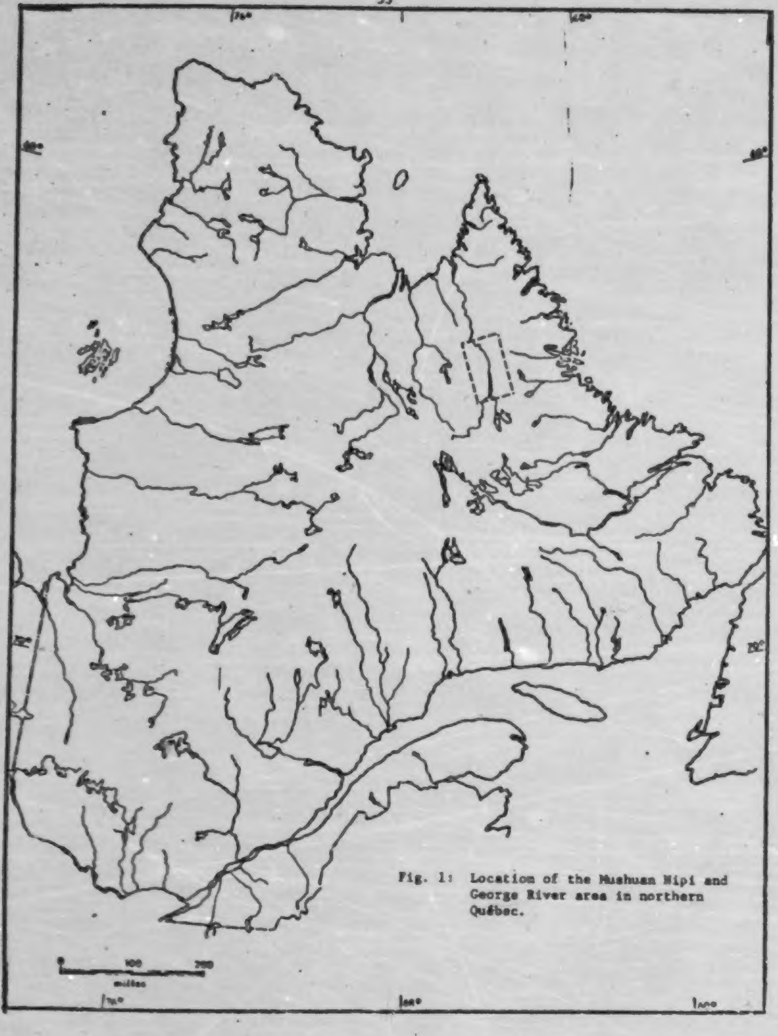
0
100
200
miles

Fig. 1: Location of the Mushuan Nipi and George River area in northern Québec.

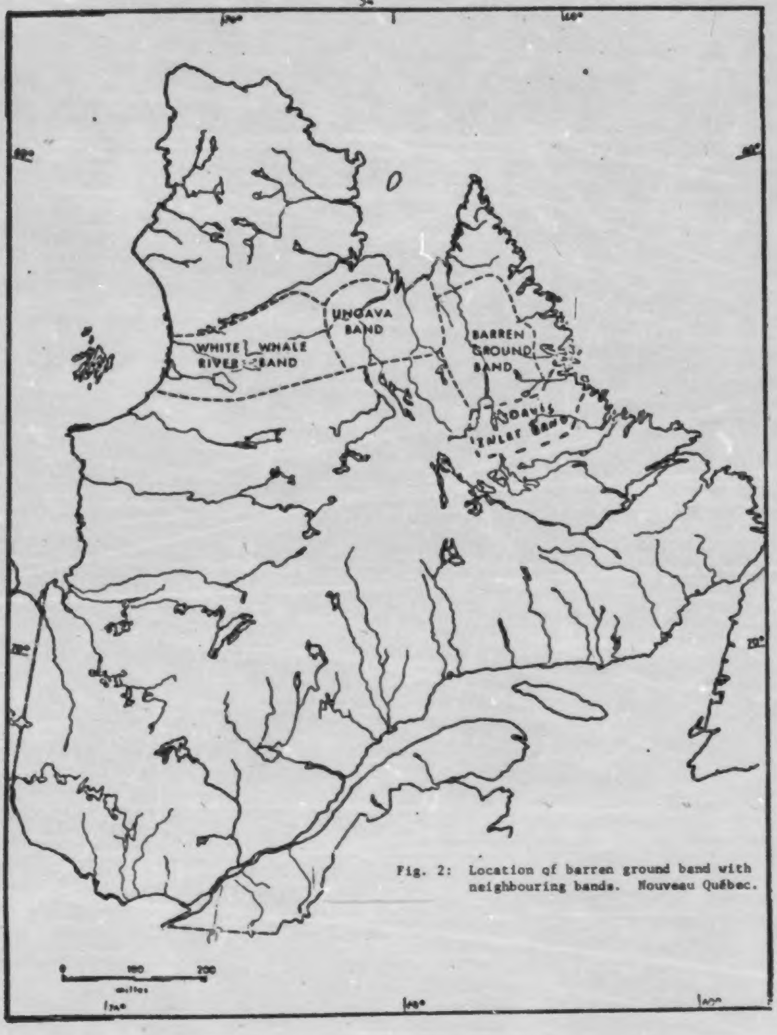
UNGAVA BAND
WHITE WHALE RIVER BAND
BARREN GROUND BAND
DAVIS INLET BAND
0
100
200
miles

Fig. 2: Location of barren ground band with neighbouring bands. Nouveau Québec.

Fig. 3: Tshinutivish 1910 campsite. (Cabot, 1912: 164)

Fig. 4: Lookout station of Mushuau Innuts. Near Mistinipi 1906 campsite. (Cabot, 1912: 248)

Fig. 5: Food and hide preparation. Mistinipi 1906 campsite. (Cabot, 1912: 228)

Fig. 6: Food storage, Tshinutivish 1910 campsite. (Cabot, 1912: 272)

Fig. 7: Women Mushuau Innuts. Mushuau Nipi 1905 summer campsite. (Hubbard, 1908: 24)

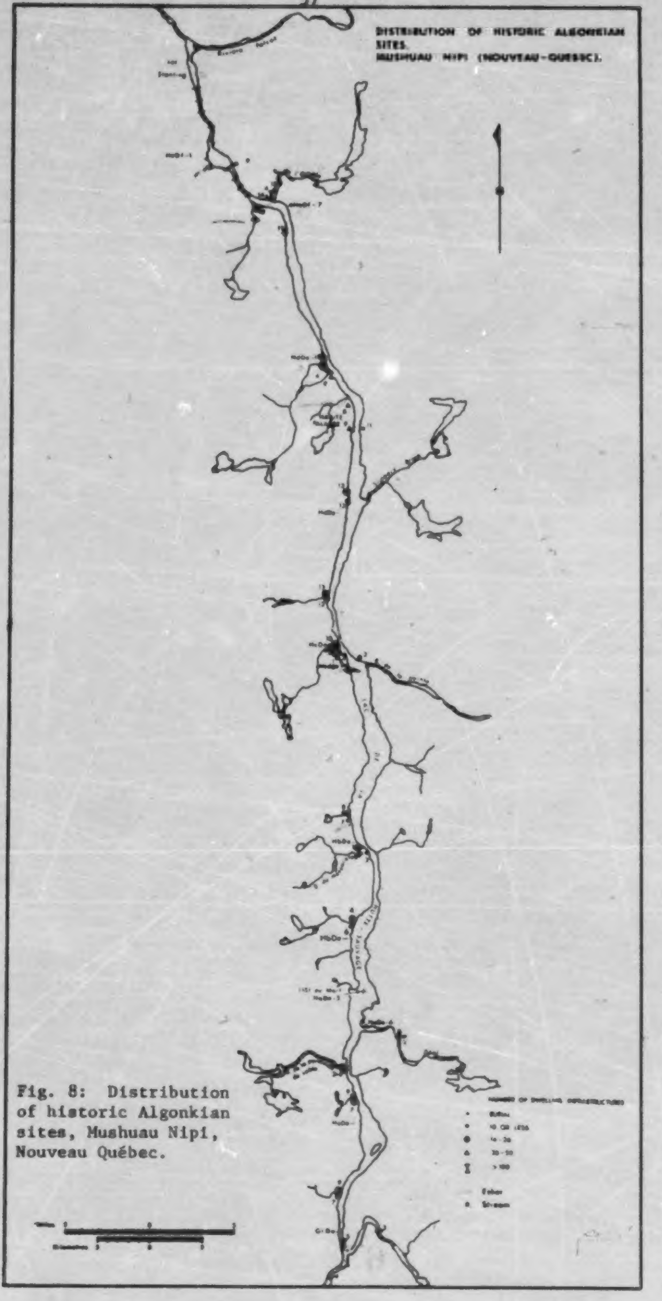
DISTRIBUTION OF HISTORIC ALGONKIAN SITES.
MUSHUAU NIPI (NOUVEAU-QUEBEC).
NUMBER OF DWELLING SUPERSTRUCTURES
> 100
Stream

Fig. 8: Distribution of historic Algonkian sites, Mushuau Nipi, Nouveau Québec.

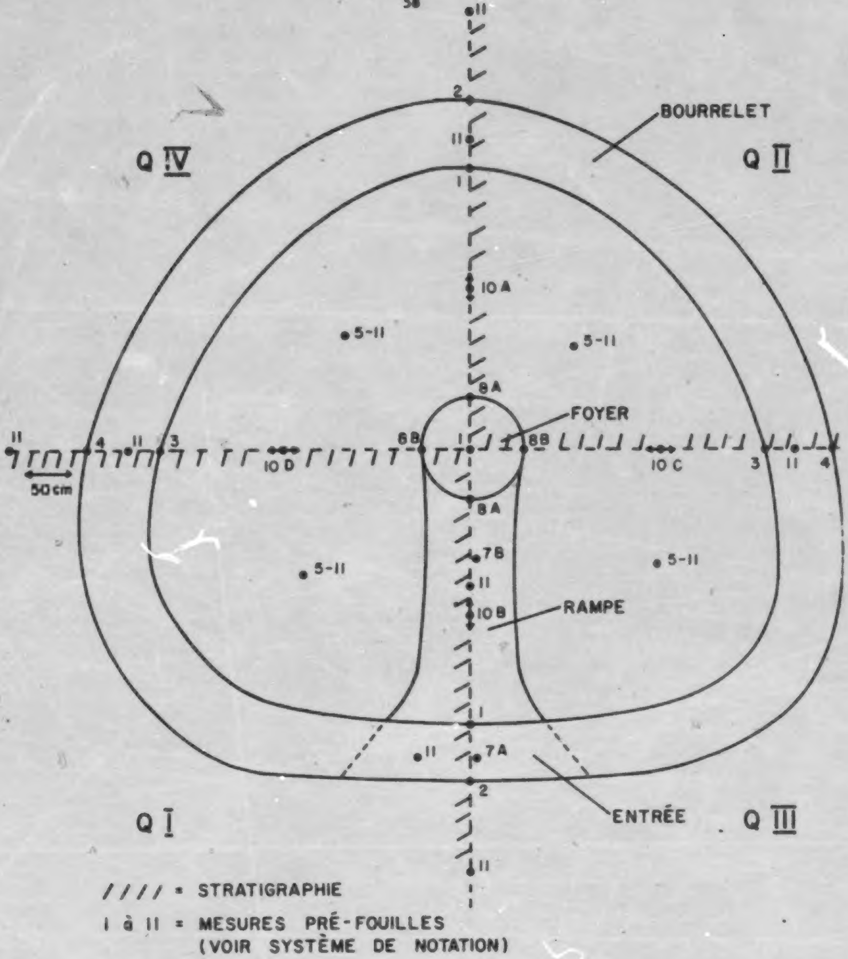
BOURRELET
Q IV
Q II
FOYER
RAMPE
ENTRÉE
Q I
Q III
10 A
10 B
10 C
10 D
8A
8B
7A
7B
5-11
50 cm
//// = STRATIGRAPHIE
1 à 11 = MESURES PRÉ-FOUILLES
(VOIR SYSTÈME DE NOTATION)

Fig. 9: Typical circular-truncated earthring with archaeological attributes.

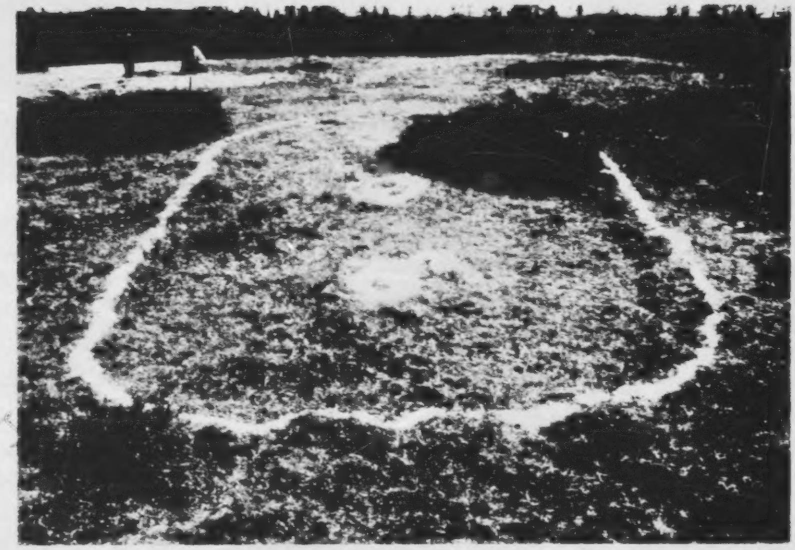

Fig. 10: Infrastructure with 3 hearths and earthring outlined by chalk, and interpreted as a ridge pole suprastructure with two entrances (Shaputuan). Site $H_gO_i$-4, terrace-3, Structure no. 82, 1973

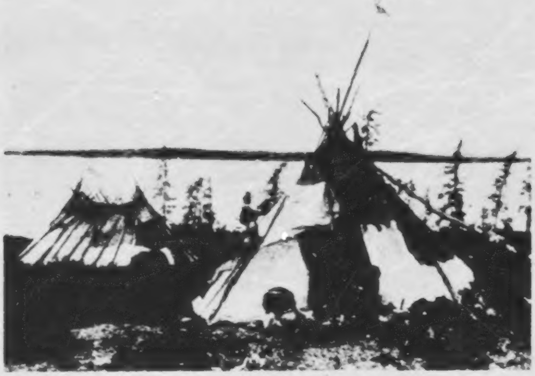

Fig. 11: Conical and ridge pole suprastructures. Montagnais Point, Resolution Lake (1905). (Hubbard, 1908: 188)

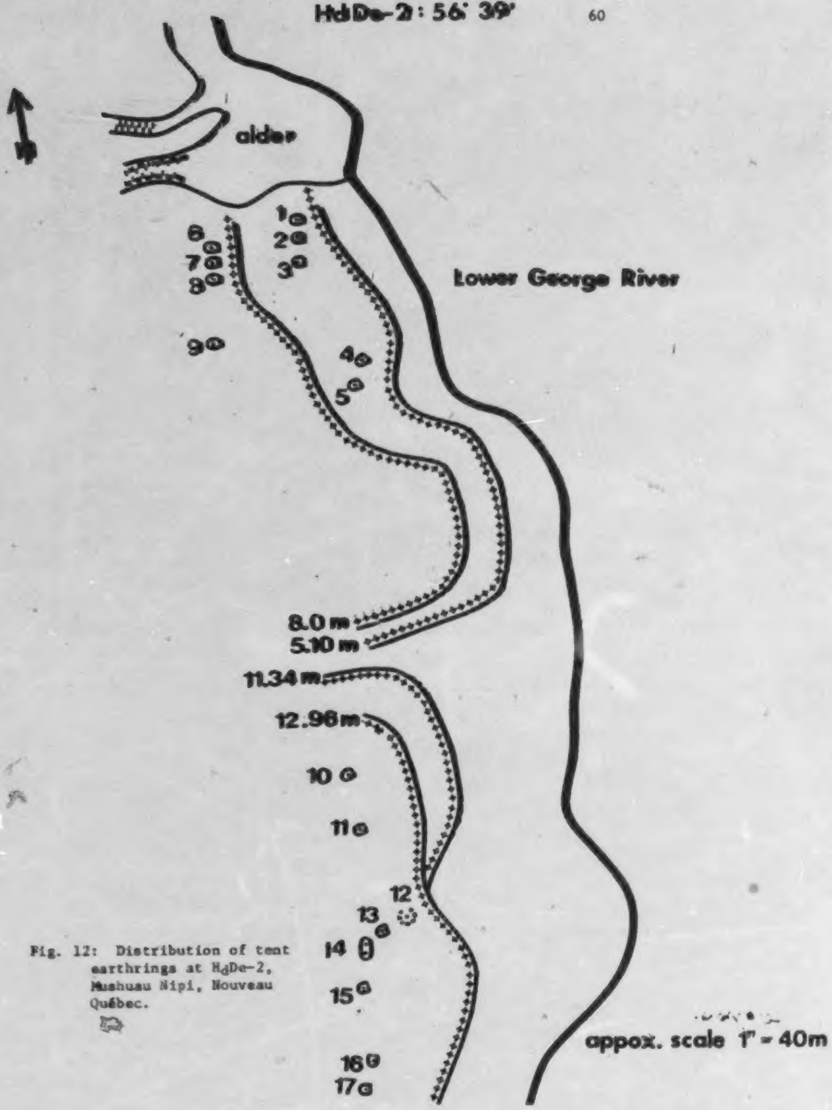
HdDe-2: 56' 39'
alder
Lower George River
1
2
3
4
5
6
7
8
9
8.0 m
5.10 m
11.34 m
12.98 m
10
11
12
13
14
15
16
17
appox. scale 1" = 40m

Fig. 12: Distribution of tent earthrings at H$_d$De-2, Mushuau Nipi, Nouveau Québec.

"One of the Wigwams Was a Large One and Oblong in Shape"

Fig. 13: Montagnais camp on the Upper George River. (Wallace, 1907: 144)

Abandoned Indian Camp on the Shore of Lake Michikamats

Fig. 14: Abandoned suprastructures: sweat tent (Nitshentshop) and conical tent (Tshistwehigantshop) Lake Michikamats (1905) (Wallace, 1907: 130)

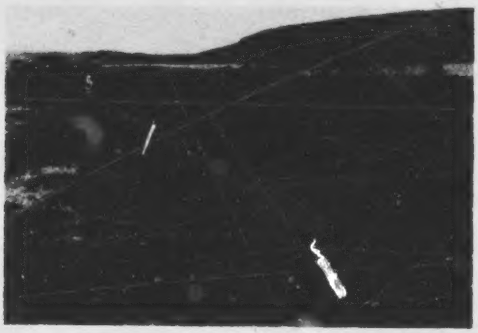

Fig. 15: Circular depression interpreted as a sweat hole. Diameter: 2.25 meters. Depth: 50cm. Structure no 92, $H_eD_f$-4 Mushuau Nipi, 1973.

# IN DEFENCE OF EDWARD UMFREVILLE

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University of Toronto

Since 1969, when Andrew Graham's manuscript Observations on Hudson's Bay was published, it has generally been assumed that Edward Umfreville plagiarized a number of passages in his The present state of Hudson's Bay (1790), including two vocabularies, from Graham, his former employer.[1] Richard Glover was the first to accuse Umfreville, in his introduction to the published version of Graham:

> It is interesting that Edward Umfreville, who had spent two years 'in charge of a post far up the Saskatchewan' when he was employed by the Nor'Westers, repeats Graham's vocabularies of the Sarsi and Fall Indians with very few changes (and some of these few differences look suggestively like miscopyings.) (Graham 1969: xxxix)

These accusations have been repeated several times by various writers, so that Umfreville is now generally looked upon as a most unreliable secondary source, and, more personally, as a thieving scoundrel who copied his employer's manuscripts in order to misuse them in furthering his own ends.

How the 'Fall Indian' (Atsina) and Sussee (Sarcee) vocabularies ever came to be attributed to Graham rather than Umfreville is a mystery. Goddard (1974:113n) assumed that Umfreville took the lists from Graham, but he observes that 'the versions that have been published from the Graham papers seem to be inferior copies'. He unfortunately does not draw the correct conclusion: Graham's version is exactly that -- an inferior copy of Edward Umfreville's vocabulary.

The proof of this claim may be presented as a number of separate points, any one of which would be strong circumstantial evidence that Graham plagiarized from Umfreville. Taken together, these points show that there is no question whatsoever that Umfreville's is the original of the two Atsina and Sarcee vocabularies.

1. Andrew Graham never mentions having met a 'Fall' (Atsina) or Sarcee Indian. He did meet a few Blackfoot Indians who made the long journey to the Bay, and his 'Black Foot' vocabulary (Graham 1969:211) includes a couple of Atsina words (along with half a dozen Assiniboine, one that is probably Arapaho, and a few words in a fifth language),[2] but these were probably collected from a Blackfoot polyglott, and Graham was quite unaware that he had ever met anyone who spoke the 'Fall Indian' language.

Edward Umfreville, on the other hand, knew the Atsina and Sarcee personally, having spent four winters in western Saskatchewan on the north branch of the Saskatchewan River.[3] He describes the various tribes as an eye-witness, and complains of the 'impropriety' of calling the Atsina Gros Ventres, 'as they are as comely and as well made as any tribe whatever' (Umfreville 1790:197). He observes that they have 'a harsh, guttural language peculiar to themselves', and it would have been natural for him to collect a few words of it in the course of his trading. Of the Sarcee he notes that they have 'a language intirely to themselves... it rather resembles the confused cackling of hens' (Umfreville 1790:199). Since one of the North West Company's interpreters knew some of the Sarcee language, he would have had no trouble in obtaining a short vocabulary.

2. A superficial examination of the two Atsina vocabularies (see the Appendix for the full vocabularies of both languages) shows that Umfreville gives forty-two words to Graham's twenty. Graham has twenty-one Sarcee words, Umfreville has thirty-eight. The English versions are virtually the same, but Graham has changed Umfreville's 'A Rabbit' to 'A Hare', and has shortened 'Brandy or Rum' to plain 'Brandy'; otherwise the glosses are identical.

3. The method used by each writer to show syllabification in Indian words is often a great help in determining authorship. James Isham (1743/1949) used apostrophes: parts of Graham's text copied from Isham betray their origin by retaining the apostrophe in the Indian words, e.g. *Puc'ca'tin'ash'-a'win* (the present given to the factor) in the description of the trading ceremony (Graham 1969:319). William Tomison and Thomas Hutchins both used commas between syllables: Graham inserted some notes on natural history from Tomison in his text without acknowledgement, e.g. *Mishe,sa,sas,awpescus* 'Large Rock Squirrel' (eastern chipmunk?) and *Kiskeman,ethtecoo*[4] 'kingfisher', both specimens presented by Tomison to the Edinburgh Museum (Graham 1969:37, 96), but again the syllabification betrays him. In a letter to Thomas Pennant (HBC Archives Z.4/1, now renumbered E.2/14, printed in Graham 1969: 381-384) Hutchins writes *Ke,nu,shu* 'pike', *Na,may,cush* 'lake trout', etc. Most writers, among them Samuel Hearne, Philip Turnor and Edward Umfreville, used hyphens to mark the syll-

ables, but Andrew Graham was almost alone in consistently leaving syllables unmarked. The Cree vocabulary Graham compiled shows no hyphens or other syllable dividers, nor do any of the natural history terms in the manuscript of his *Observations* that is in his own handwriting.[5] Graham also omits the hyphens Umfreville inserted to show the syllabification of the Atsina and Sarcee words.

A comparison of Umfreville's Atsina and Sarcee vocabularies with the modern languages shows that the hyphens have linguistic significance -- they are not inserted at random, as they would have to have been if Graham's was the original. In *Nub-o-on-er* 'A Pair of Shoes' (actually 'my shoes', modern Atsina /newʔóhonoh/) the first two hyphens indicate breaks in the word that Umfreville heard but could not symbolize adequately, and his spelling *Uk-ke-er* 'two' is a much better representation of modern Sarcee /ìkívi/ than Graham's *Ukkeer*.

4. Neither version is completely free of errors,[6] but it is noteworthy that Graham's version is never more nearly correct than Umfreville's. Umfreville (or his printer) is responsible for *Hidth-er* 'Gunpowder' in the Atsina list in place of *Kidth-er* (old Atsina /kʔítθee/, modern /čʔítee/), but it was Graham who misread Umfreville's *Utce-ee* 'An Arrow' (actually 'arrows', modern Atsina /ʔóciih/) and *Cu-neese-nun-nee* 'ten' (modern Sarcee /gunìsnáni/) as the nonsense words *Uteeee* and *Cuneeseuunnee*.

5. Graham must have copied Umfreville's book after it was published in 1790. The vocabularies could not have existed

prior to Umfreville's trip to Saskatchewan after leaving the Hudson's Bay Company's service, so Graham could hardly have obtained it while Umfreville was his assistant. It is also not very likely that the two men got together to exchange notes after Umfreville's retirement from the fur trade in 1788. Umfreville went to live in London, while Graham was living in Edinburgh.[7]

Graham probably copied out the vocabularies into parallel columns, accidentally reversing the order of two entries ('arrow' and 'bow'), and later recopied them into separate lists (in paragraph form) in his book of observations. This copying and recopying led to an interesting error on Graham's part. Umfreville did not collect six Sarcee words and omitted one word, 'rabbit', in the Atsina list. Graham had pretensions as a naturalist -- he collected specimens for the Royal Society and tried to use scientific terminology in describing them in his manuscripts. In fact, it was Graham's material that led to the publishing of an article by Daines Barrington on the distinction between rabbits and hares, in 1772. When Graham copied Umfreville's vocabularies he changed the gloss from 'A Rabbit' to 'A Hare', and, perhaps preoccupied in making this alteration, failed to notice that he filled in the Indian word from the wrong column. He gives Mustinge as the Atsina word for 'hare' -- a word that is phonologically impossible in that language. It appears correctly in Umfreville's list as the Assiniboine for 'rabbit' (modern /mastʃča/). No one working directly from an informant could make such a mistake.

6. Most people, whether fur traders or linguists, use a consistent system of transcription no matter what language they are working on. When Graham collected his 'Black Foot' vocabulary, he wrote the Atsina word for 'knife' (modern /wóoθo/) as *Wath* and 'gun' (/kocíyo/) as *Cattee*. In his 'Fall' vocabulary (on the same page in the manuscript) he writes *Warth* and *Cutsier*.

7. Graham must have made more copies of his manuscript than now exist: E.2/6, purchased by the HBC as recently as 1926 from a bookseller, is called 'Book the Second' on its title page, but volume one has not been found. In 1797 the London Committee of the HBC ordered Joseph Colen to return the copy of Graham's *Observations* he had taken with him to York Factory;[8] it is not known whether he ever did so. None of the extant manuscripts could have served as the parent of all the others as far as the text of Isham's Indian Leader's Speech is concerned (see Appendix), nor is the surviving Graham copy of Umfreville's vocabularies likely to have been made directly from *The Present State of Hudson's Bay*. Thus there must have been at least two other copies of Graham's *Observations* (the missing half of E.2/6, and the parent manuscript, *G). Neither of these could have contained the Umfreville vocabularies, for E.2/6 is dated 1769, and *G must have existed before the first known derivative of it (which is E.2/4-5, also dated 1769). Of the dozen known or assumed copies of Graham's book, the only one to contain Umfreville's Atsina and Sarcee vocabularies is E.2/12, the last one Graham made. This volume was

still in Graham's possession after Umfreville's book was published, and was probably sent by him to the Hudson's Bay Company in 1793.

8. Little is known about Edward Umfreville outside of his fur trade career.[9] He was probably born in the early 1750's, and a rather insecure family tradition has him coming from Colchester, fifty miles northeast of London. The same source suggests that he was buried at Great Missenden, Berkshire, again no great distance from the capital. Of Andrew Graham we know a little more. He was probably born in Edinburgh, since he spent his leave there in 1769-1770, and married an Edinburgh girl. The marriage register describes both bride and groom as residents of Old Greyfriar's parish, Edinburgh.[10] On his retirement five years later, Graham returned to Edinburgh, and he lived in or near the city until his death at Prestonpans in 1815.

Umfreville (1790:202) tells us that he spells Indian words 'according to an Englishman's pronunciation'. He spoke the standard Southern British English of his time -- he was born and died within fifty miles of London, and was reasonably well educated, since the HBC hired him as a 'writer'. Long before Umfreville's time East Anglia and London had lost the consonant r (or changed it to a centralizing glide) after vowels (before a consonant or word boundary).[11] He is therefore able to use the spelling device of vowel + r in all his vocabularies to represent pure vowels, and gives no words in which r stands for a consonant. Graham, on the other hand, like any good

Scot, pronounced English r in all positions as a rolled /r/. He writes r three times in his Chipewyan vocabulary (where it functions as a consonant), but nowhere else except in the material he plagiarized from the Englishman Edward Umfreville.

On all these points: (1) the opportunity to collect Atsina and Sarcee; (2) the comparative length of the vocabularies; (3) the lack of relevant hyphens in Graham's version; (4) the relative number of errors in each; (5) the attribution of the word for 'rabbit' to the wrong language; (6) the consistency of orthography; (7) the fact that Graham could have copied Umfreville's book after publication while Umfreville never saw Graham's manuscript E.2/12; and especially (8) the non-Scottish character of the spelling system; -- Umfreville's vocabularies come out ahead of Graham's, and we must assume that Graham was the plagiarist.

What of the other parts of Umfreville's book and Graham's manuscript? As Williams and Glover observed when editing Graham, there are passages which repeat Umfreville word for word, and other sections which because of subject matter or choice of phrasing must have come from a common source. Both men had read the books of their predecessors in Hudson Bay: Henry Ellis's Voyage to Hudson's-Bay (1748), Joseph Robson's Account (1752), and Arthur Dobbs's attack on the Hudson's Bay Company (1744), as well as the report of the Parliamentary inquiry into the Company's affairs in 1749. Quotations and information from all these sources appear in both books, sometimes, but not always, the same information and in the same words.

Richard Glover has argued (Graham 1969:xxx) that Andrew Graham never plagiarized -- that plagiarism consists of copying with the intent to publish (which Graham never did) -- but the Oxford dictionary defines plagiarize as 'to take and use as one's own the thoughts, writings, or inventions of another', and this Graham was most certainly guilty of. Large sections of his manuscripts are copies of other men's journals (only Hearne's is printed in Graham 1969). The chapter on Eskimos is largely a paraphrase of David Crantz's History of Greenland with a few of Graham's own experiences added for contrast. To judge from the orthography of the Cree words, much of the natural history sections (far more than Glover will admit) originated with Thomas Hutchins.

There are several long passages in Umfreville that are virtually identical to Graham. Unless we assume that Graham was allowed to read The Present State of Hudson's Bay while it was still in manuscript form -- a most unlikely state of affairs -- it must be admitted that Umfreville was the one who did the copying. The plagiarized sections are listed in Table 1.

After page 64 of Umfreville 1790 I have found no direct quotations from Graham. There are some sections that are reminiscent of Graham's text, but they are always matched by an actually copied paragraph in the first part of the book.[12] Did Umfreville get tired of copying from Graham's manuscript?

| Umfreville | | Graham | Subject Matter |
|---|---|---|---|
| 1790 | 1954 | 1969 | |
| 11 | 6 | 3 | Temperature range in Hudson Bay |
| 12 | 6-7 | 4 | Brandy and other liquor freezing; cask of water freezes in 48 hours |
| 13-21 | 7-11 | 308-313 | Death of John Farrant and James Tomson, 1772 |
| 22 | 11 | (cf. 313-315) | Hutchins's cure of James Ross; collection for Ross |
| 31-32 | 17 | 155-156 | Effects of excessive liquor |
| 32-33 | 17-18 | 156 | Stabbing incident, 1766 |
| 35-36 | 18-19 | 143-144 | Natives of Hudson Bay; 'Country distemper' and venereal disease |
| 42-43 | 22 | 159-160 | Cree religion |
| 43-45 | 22-23 | 169-170 | Cree government |
| 45-53 | 24-26 | 196-200 | Samuel Hearne's journal |
| 53-55 | 27-28 | 166-167 | Months in Cree; number system |
| 56-63 | 28-31 | 316-319 | Annual trading cycle; trade ceremonies |
| 63-64 | 31-32 | 323 | Indian Leader's Speech (see Appendix) |

Table 1. Identical passages in Umfreville and Graham

All of the plagiarized material is found in fifty-four small pages, and Umfreville's own comments and observations take up part of this space. Graham's manuscripts contain enough material to fill half a dozen books the size of Umfreville's: why did he stop copying?

The answer to this question can be found by asking another: when was The Present State of Hudson's Bay written? The foreword is dated 'London, June 17th, 1789'; although the book was published in 1790, Umfreville had finished it more than six months earlier.

We know in detail what Edward Umfreville was doing during the preceding period, and he could have had almost no free time for writing until his arrival in London in January, 1789. On January 23 he wrote the Committee of the Hudson's Bay Company to ask for a job. The letter was read at their February 11 meeting, and no doubt a reply was sent to Umfreville later in the month. In April Umfreville wrote the Committee again, and this second letter was read at a meeting held on April 22. A month later the Company wrote William Tomison (who had recommended Umfreville) to say that he had applied to them, but they were unable to agree to terms.[13]

Umfreville must have received the bad news by the end of April, 1789, and seven weeks later he had completed his book attacking the Hudson's Bay Company. If he started writing as soon as the Company rejected him, and worked six days a week until he was finished, he would have to have written more than five pages a day, at the same time doing all the background reading and research required. This, I believe, was far beyond his capabilities. Umfreville must, then, have started writing before he was turned down by the Committee in April 1789. But would a man looking for a career with the Hudson's Bay Company put together a manuscript which, if it were ever discovered, would lead to his instant dismissal and the enmity of a powerful company? Umfreville was seeking a position as governor of one of the Bay-side forts: he wanted to be one of the men who 'reign as absolute in their petty commands as Eastern Nabobs' (Umfreville 1790:106).

A closer examination of The Present State of Hudson's Bay shows that the book was written over a long period of time. In the first two sections (on the geography and inhabitants of the Bay), which contain all the paragraphs plagiarized from Graham, the dated episodes mention nothing that happened after Umfreville was captured by the French in 1782. In the next three chapters (on the HBC and its trade) he compares the statistics of 1749 (from the Parliamentary report of that year) with those of 1782, the last year he worked for the Company, and describes the smallpox epidemic of 1781 as a recent event. He also makes the following telling remark: 'we recollect that this country has been in the hands of an incorporated Company for upwards of one hundred and thirteen years' (Umfreville 1790:102). Since the Hudson's Bay Company was incorporated on May 2, 1670, Umfreville's comment should refer to 1783, not 1789, when he is supposed to have been writing. All of the above is printed as one long chapter with occasional subheads.

We then find two short chapters reprinting letters to the Morning Chronicle describing the capture of York Factory and Churchill. Although the letters, originally published in April, 1783, are anonymous, there can be little doubt that Umfreville was the writer. Up to this point, more than 60% of the way through the book, there is nothing that could not have been written in 1783, and several passages that must have been.

On August 24, 1782, Umfreville had been captured at York Factory by the French fleet under La Pérouse; in keeping with its usual policy, the Hudson's Bay Company promptly cut off his salary.[14] He was taken to France, probably to Dinan. The London Committee received several letters from 'Sundry Servants, Prisoners at Dinan Castle France' in October and November, 1782, presumably the same group of 'Sundry Servants' (no doubt including Edward Umfreville) who again wrote on about January 18, 1783, from on board the Diligent man of war at Portsmouth.[15] On February 27 Umfreville collected what there was of his back pay, but could not get the Company to part with any salary for the time he was in French hands.[16] At this point in his life Edward Umfreville was more opposed to the Hudson's Bay Company than at any other time before or after. He wrote two letters to the Morning Chronicle proclaiming the extreme cowardice of the governors of York and Churchill Forts, and promptly set sail for Quebec. He spent the winter of 1783-84, he tells us, 'near Montreal', but makes no mention of what he was doing at the time. He had enough money to live on, having collected £42 14s 2d from the Company on his release from France (HBC Archives A.15/14, 80), and must have devoted his time to writing a diatribe against the Company that had, in his eyes, used him so badly. While he could hardly have written a 230-page book in seven weeks, it was quite possible for him to have written half that amount over a period of more than a year when he had little else to do.

In May, 1784, Umfreville joined the North West Company. Early in June he was sent to Lake Nipigon as leader of an exploring party to find a new route to Lake Winnipeg.[17] He went directly from this task to trading on the North Saskatchewan River, where he spent the winters of 1784-1787 (Umfreville 1790:146). In the spring of 1788 he decided to leave the Nor'Westers. He started from the Alberta-Saskatchewan border on May 14, reached Lake Superior in July, and arrived at Montreal on September 15. Ten days later he set off for New York, which he reached on October 10, 1788 (Umfreville 1790:221-230). From there he set sail for England, and on his arrival in London began his unsuccessful attempt to obtain a suitable position with the Hudson's Bay Company.

The second half of Umfreville's book has a completely different tone from the part he wrote in 1783. Much of it is simple description of the country and people he saw while working for the North West Company. When he mentions the Hudson's Bay Company it is to refer to the incorruptible honesty of the employees and the quality of their goods, and to compare them most favourably with the Canadians. He ends by proposing (Umfreville 1790:218-219) that the Nor'Westers should be made 'truly respectable' by amalgamation with the Hudson's Bay Company -- a strange proposal from a man who was a hundred pages earlier denouncing the older company in the strongest possible terms. It is obvious from the phrasing of the second part of the book that most of it (including the Indian vocabularies) was written on the North Saskatchewan River in 1787. On being turned down by the HBC Committee, Umfreville had only to make a few final revisions and write the introductory matter.

Edward Umfreville was an honest writer by eighteenth-century standards: he stated his sources when he quoted them. He mentions Ellis, Robson, Carver and 'the patriotic Mr. Dobbs' by name. He gives a summary of Samuel Hearne's journal,[18] prefaced with the note 'The person employed in this business gives the following account of his expedition... These are his words:' (Umfreville 1790:45) and at the end adds some complementary remarks about Hearne's abilities.

When Umfreville tells the story of James Ross's adventure on an ice floe, he mentions that Thomas Hutchins was the doctor at York Factory who treated him for frostbite. In the Preface, he adds that the account was obtained from 'A gentleman who resided there in the capacity of Surgeon... (and) made many useful experiments at York, and Albany Forts' (Umfreville 1790:10) -- this of course was the same doctor.

Thomas Hutchins made many natural history observations at York and Albany, and in 1783 became Corresponding Secretary of the Hudson's Bay Company at its London headquarters. Umfreville must have met Hutchins in London in 1783 when he was trying to collect his back pay -- he could hardly have avoided it, as Hutchins was the Committee's voice in dealing with its employees.

Williams and Glover have demonstrated that Thomas Hutchins and Andrew Graham freely copied material from each other; it is often impossible to determine with which man a paragraph originated, since they were together at York Factory for a time and collaborated in making experiments and collecting specimens throughout their careers. Graham's editors have concluded that most of the material they published in Observations on Hudson's Bay was originally his, but they may have been misled by their reliance on manuscript E.2/12, a copy Graham made after Hutchins's death. Certainly the spelling of the Cree animal names in some of the other copies resembles Hutchins's known work more than it does Graham's. One manuscript (E.2/10) was at some time prepared for publication (presumably with Hutchins to be named as author), and was quoted by John Latham as Hutchins's work. Another (E.2/13, the parent of E.2/10) has a note on the flyleaf which says 'This book was written by Mr. Hutchins' (Graham 1969:352-361).

When Edward Umfreville was in London in the spring of 1783, Thomas Hutchins must have allowed him to copy from E.2/10, as he had allowed others to do. During the following year, Umfreville worked up these notes into the first half of his book, The Present State of Hudson's Bay, and acknowledged his debt to Hutchins in the Preface. He would have had little reason to suspect that the Honourable Company's Corresponding Secretary was passing off another's work as his own. Everything in Umfreville's book is either his own thoughts or an acknowledged quotation -- he was no plagiarist.

In his introduction to Graham (1969:xxxvi) Richard Glover comments 'To publish another man's book while that man still lived to resent the literary theft would be impossible'. He suggests that this is the reason that Hutchins never released E.2/10 for publication. Yet Umfreville published forty pages of this manuscript without any observable reaction from Graham. If Graham had been the true and only author of the material Umfreville quoted, it would have been natural for him to protest. But he does not -- instead, he in turn copies two vocabularies from Umfreville. Nor does he show any hostility toward Thomas Hutchins. He may never have seen Latham's work, though it is one he would have consulted if he knew of its existence, but he certainly read Umfreville, and could figure out easily enough that Hutchins had provided the author with his information. Yet in E.2/11 (a companion volume to E.2/12), Graham ends with the note 'This Worthy Gentleman dyed in London in June 1790. Corresp$^{g}$. Sec$^{y}$. to the HBay C$^{o}$.' -- an unlikely epitaph for a man who had stolen the credit for Graham's life work. It seems to me that there is remarkably little in *Observations on Hudson's Bay* that Graham could honestly claim as his own. The volumes are full of long extracts from the journals of Henday, Cocking, and others, and observations on natural history that (in part, at least) are more likely to have been made by Thomas Hutchins than Andrew Graham. Perhaps the reason Graham never complained about others' plagiarism is that this would have exposed his own, far more extensive, copying. Or perhaps he did not complain because the largest part of the material quoted by Umfreville was not his in the first place.

# APPENDIX

## THE ATSINA AND SARCEE VOCABULARIES

These vocabularies, collected by Edward Umfreville on the North Saskatchewan River near the Alberta border, probably in 1786 or 1787, are printed here exactly as they appear in his *The Present State of Hudson's Bay* (1790), together with the copy made by Andrew Graham in his manuscript E.2/12 (1793). I have added the modern equivalents where possible.

### FALL INDIAN (ATSINA) VOCABULARY

1 An Eye, *Nun-nec-so-on*, *Nunnecsoon* (1)

2 A Pair of Stockings, *Nun-nor-tor*, *Nunortur* (2)

3 A Shirt, *Ne-weed-thu-it*, *Neweedthuit* (3), /neb$^{y}$íiθuut/ 'my shirt'

4 A Knife, *Warth*, *Warth* (4), cf. *Wath* in Graham's 'Black Foot' vocabulary (1969:211), /wóoθo/

5 A Tobacco-pipe, *Pe-chou-on*, *Pechowon* (5), /ʔíicóoo/; the initial *P-* is either a mishearing on Umfreville's part or a printer's error.

6 A Hat, *Ti-u-it-te-ter*, *Tiuitteter* (6), (modern /wotóʔ/)

7 A Pair of Shoes, *Nub-o-on-er*, *Nubooner* (7), /newʔóhonoh/ 'my shoes'

8 Tobacco, *Chees-ou-on*, *Cheesouon* (8), /cíiθóówoo/

9 A Rabbit, *...*, *Mustinge* (9) 'A Hare'; Umfreville has Assiniboine *Mus-tinge* (/mastį́ča/).

10 A Gun, *Cuts-i-er*, *Cutsier* (10), cf. *Cattee* in Graham's Blackfoot, /kocíyo/

11 Brandy or Rum, *Nuts*, *Nuts* (11) 'Brandy', /níc/ 'water'

12 Gunpowder, *Hidth-er*, *Hidther* (12), /čʔítee/; initial *H-* is a misprint for *K-* (old Atsina /kʔítθee/).

13 Ball, *Cuts-i-er-notce*, *Cutsier-notce* (13), /kocíyonoc/ (literally, 'gun arrow')

14 A Hatchet, *Oh-en-orce*, *Ohenorce* (14), /ʔohʔonóos/

15 A File, *Oh-er-er*, *Oherror* (15); see footnote 6.

16 A Dog, *Hudth-er*, *Hudther* (16), /ʔót/ (old Atsina /ʔótθe/)

17 Fire, *U-sit-ter*, *Usitter* (17), /ʔisítae/

18 An arrow, *Utce-ee*, *Uteeee* (19!), /ʔóciih/ 'arrows'

19 A Bow, *Bart*, *Bart* (18!), /béete/

20 A Pot, *In-au-un*, (not in Graham), /ʔinooo/ (not attested, but expected reflex of Proto-Algonquian *wela·kana)

21 Beads, *Can-ar-ti-u*, *Canartiu* (20)

22 Cloth, *Nau-odth-i-u* (the remaining Atsina words are not in Graham's copy)

23 A Horse, *Wau-ce-hoth*, /ʔiwósiihóoθ/ (literally 'elk dog')

24 A Kettle, *Ma-ja-a-tian-au* (perhaps 'metal' + 'pot')

25 Shot, *Cha-chu-chi-o-noche*

26 An Ice Chizzel, *Thou-wau*

27 A Handkerchief, *Same as shirt*

28 A Scraper, *Won-ut*

29 A Comb, *Char-i-uh-ay*

30 A Pair of Mittins, *Node-thoth*, /notóoθ/

31 Paint, *i.e.* Vermillion, *Na-olth-i-er-in-o*, likely a misprint for *Na-otth-*; the last two syllables are /-inow/

32 An Awl, *Bay*, /béi/ 'needle'

33 Rings, *Tha-ith-e-te-an*

34 A Powder Horn, *Ne-nis*, /nííni s/ 'horn'

35 One, *Kar-ci*, /čééθíy/ (old Atsina /kéésíy/)

36 Two, *Neece*, /nííθ/

37 Three, *Narce*, /néeθ/

38 Four, *Ne-an*, perhaps a misprint (carrying over the initial *N-* from 'two' and 'three') of *Ye-an*, /yéén/, but cf. Goddard 1974

39 Five, *Yau-tune*, /yóótón/

40 Six, *Ne-te-ar-tuce*, /nɛ́it$^{y}$ootos/

41 Seven, *Ne-sar-tuce*, /nííθootos/

42 Eight, *Nar-swar-tuce*, /néeθootos/

43 Nine, *An-har-be-twar-tuce*, /ʔeenheebítootos/

44 Ten, *Met-tar-tuce*, /bítootos/ (old Atsina /métootos/)

SUSSEE (SARCEE) VOCABULARY

1 An Eye, *Se-nou-woh*, *Senowwoh* (17), misprinted as *Senowwok* in Graham 1969, /si[illegible]náyàʔ/ 'my eye'

2 A Pair of Stockings, *Sist-ler*, (not in Graham)

3 A Shirt, *Si-cho-we-cher*, (not in Graham)

4 A Knife, *Marsh*, *Marsh* (18), /mas/

5 A Tobacco-pipe, *Mis-tu-tey*, (not in Graham)

6 A Hat, (not in Umfreville or Graham)

7 A Pair of Shoes, *Sis-cau*, (not in Graham), /-káʔ/ 'shoe, moccasin'

8 Tobacco, *Me-cut-chin-er*, (not in Graham), (modern /ná'lsgòni/)

9 A Rabbit, (not in Umfreville or Graham)

10 A Gun, *Til-te-tha*, *Tiltetha* (19), (cf. 'A Bow' -- the old word for 'bow' now means 'gun')

11 Brandy or Rum, *To*, *To* (20) 'Brandy', /tú/ 'water'

12 Gunpowder, *Til-te-the-tus-er*, (not in Graham), 'gun' + /-t'àsàʔ/

13 Ball, *Til-the-the-tan-ny*, (not in Graham), 'gun' + ?; misprint for *Til-te-the-*

14 A Hatchet, *Chilthe*, (not in Graham), /tsíł/ 'axe'

15 A File, *Til-te-the-te-co-sey*, (not in Graham), 'gun' + ?; see footnote 6.

16 A Dog, *Tley*, *Tlev* (11), misprinted as *Hev* in Graham 1969, /tłi/

17 Fire, *Coo*, *Coo* (12), /kuʔ/

18 An arrow, *Hil-tun-ney*, *Hiltunney* (14!)

19 A Bow, *Tar-ney*, *Tarney* (13!), /dóóní/ 'gun'

20 A Pot, (not in Umfreville or Graham)

21 Beads, *Uce-chit-ler*, *Ucethitler* (21)

22 Cloth, *Che-we-sey*, (not in Graham)

23 A Horse, *Che-che-nun-to-er*, *Chechenuntoer* (15), (modern /tłítš'a/, /istłí/)

24 A Kettle, *Us-saw*, *Ussau* (16), /ásòʔ/ 'pot'

25 Shot, (not in Umfreville or Graham)

26 An Ice Chizzel, (not in Umfreville or Graham)

27 A Handkerchief, *Sees-ul-er*, (not in Graham)

28 A Scraper, *U-wil-twey*, (not in Graham), /ʔóɣùłtiʔ/

29 A Comb, *Char-uc-ee-chev*, (not in Graham)

30 A Pair of Mittins, *Se-ut-ee-ser*, (not in Graham)

31 Paint, i.e. Vermillion, U-ti-el-ee-cher, (not in Graham)

32 An Awl, Chalthe, (not in Graham), /gááɬ/

33 Rings, See-lar-o-tar-ny, (not in Graham)

34 A Powder Horn, (not in Umfreville or Graham)

35 One, Ut-te-gar, Uttegar (1), /tɬìk'a-zá/ 'one only', /átɬìká-gò/ 'one at a time'

36 Two, Uk-ke-er, Ukkeer (2), /ìkíyi/

37 Three, Tau-key, Taukey (3), /tóók'i/

38 Four, Ta-chey, Tachey (4), /díítš'i/

39 Five, Cu-cel-ter, Cucelter (5), (modern /gúùt'áá/)

40 Six, Uce-tun-nee, Ucetunne (6), /gùstóni/

41 Seven, Che-che-ta, Checheta (7), /tšìštš'ídi/

42 Eight, Tar-titch-ey, Tartichey (8), /tɬàšdíítš'i/

43 Nine, Ke-cutch-ee-gar, Kecutcheegar (9), /tɬìk'úyawá/

44 Ten, Cu-neese-nun-nee, Cuneeseuunnee (10), /gunìsnáni/

AN INDIAN LEADER'S SPEECH TO THE ENGLISH FACTOR

The following version of the speech is a composite, based largely on E.2/9, but occasionally on the other manuscripts when they agree against E.2/9. This reconstructed text is not exactly the same as the original that James Isham wrote -- Graham seems to have made some changes when he copied it -- but it is probably the version that appeared in *G, Graham's missing master copy.

The Cree and English texts are aligned as far as possible, but since they do not match exactly, an occasional word may have been assigned to the wrong line. Isham's manuscript, E.2/1 (identical to the published version, Isham 1949), is abbreviated as *I*. The Graham manuscripts, E.2/5, 7, 8, 9, 10, 12, and 13, are identified by their volume numbers (e.g. E.2/5 is *5*), and Umfreville's 1790 version is *U*. (The remaining manuscripts -- Isham's E.2/2 and 3, and Graham's E.2/4, 6, and 11 -- do not contain the Indian Leader's Speech.) The only other abbreviation used is *NP* 'no punctuation after the word cited'.

Ketabachemout[1] neponuck[2] mechet[3] Ethinue[4]
You told me last[5] year[6] to bring many Indians,[7]

[1]*Katabachemout* 10. [2]*neponock* 10, 13. [3]*mechet,* 13. [4]*ethinue* 5; *Ethinue,* 9, 12; *Ethinuc* 10. [5]*Last* I. [6]*Year,* 10. [7]*Indians;* 7, 8; U has *Indians to trade, which I promised to do;*.

cattapeche[8] whitcomow,[9] queapamow[10] nema enktasken[11] mechet[12] unskeneck;[13]
you see[14] I have not lyed,[15] here are[16] a great many young men[17] come with me;[18]

[8]*catapeche* 5; *cattapechi* 10. [9]NP 10, 12, 13; *whitemow,* 5; *whitcomou* 10. [10]*queapmow* 5; *qucapamou* 10. [11]*kithiasken,* 5; *inktaskin* 10. [12]*meche* 10, 13. [13]*wiskeneck;* 5; *uwskeneck* (NP) 10; *unskeneck,* 13. [14]*See* I. [15]*Ly$^{d}$:* I; *lied,* 5; *lied;* U. [16]*is* I. [17]*Men* 8, 9, 10. [18]*me,* I, 5, 10; *me.* 7, 8.

curshewathemow,[19] curshewathemow[20] entetwan!
use[21] them kindly,[22] use[23] them[24] kindly[25] I say![26]

[19]NP I, 10; curshewethemou 10; curshewethemow, 13. [20]curshewathemow, 9; curshewethemou 10; curshawethemow, 13. [21]Use 7, 8. [22]Kindly! I. [23]Use 7, 8; U lacks the second use them kindly. [24]Y$^{m}$ I. [25]Kindly I; kindly, 10. [26]say. 7, 8; say; U.

(no Cree)

Let[27] them trade good goods,[28] let[29] them trade good goods[30]
I say![31]

[27]let 5, 10, U; I has give them good goods. [28]NP 12; goods; U. [29]Let 9, 12, 13; I has give them good Goods. [30]Goods I; goods, 10, U. [31]say!-- I; say. 7, 8.

addamon[32] peponuck,[33] awemetsun[34]
we[35] lived[36] hard last[37] winter[38] and hungry,[39]

[32]adamon 10. [33]NP 5, 10. [34]awemettsun, 5. [35]We 7, 8, 12, U. [36]Liv$^{d}$ I. [37]Last I. [38]winter, 7, 8, 13. [39]I has in want.; hungry; 8, 9, 13.

wache[40] appashashen,[41] cawquapahat[42] pethko[43]
the powder being short measure[44] and bad,[45]

[40]nacha 10; nache 13. [41]appishiashen (NP) 5; appasharshin (NP) 10; misprinted as appachashen, in Graham 1969. [42]cawquapahan 5; cawquapachat 10. [43]pethko, 5; petheo 10. [44]measure, 13. [45]bad; U; 12 has the powder being expended (NP).

wache[46] appashashen,[47] cauquapahat[48] pethko[49] entetwan![50]
being short measure and bad[51] I say![52]

[46]wacha 10. [47]appishiashen (NP) 5; appashem (NP) 10;

*appashen,* 13. [48]*cawquapahat* 9, 13. [49]*pethco* 10. [50]*Entetwan!* I, 9. [51]I lacks *being short measure and bad;* 12 has *the powder being expended;* *bad,* 10, U. [52]*say!--* I; *say.* 7, 8.

wittomow[53] ketuskerogan[54] cuttasockaskena[55] quapahigan;[56]
Tell[57] your Servants[58] to fill the measure[59]

[53]*whitimow* 5; *wittomo* 9; *wittomou* 10; *Wittimo* 12. [54]5 has *tatawan mistegush;* *ke tuskethogan* 10, 13; misprinted as *Ketusker ketusker ogan* in Graham 1969. [55]*cut a sockiskenna* 10; *cutasockaskena* 13. [56]*quapahigan,* 5, 12; NP 10. [57]*tell* I, 5, 10. [58]*servants* U; 5 has *tell the trading Englishman*. [59]*measure,* 7, 8, 10, U.

(no Cree)
and not to put their[60] thumb[61] within the brim;[62]

[60]*his* 5. [61]*finger's* I; *thumbs* 10, U. [62]*Brim,* I; *brim.* 7, 8; *brim!* 9; *brim,* 13; 12 lacks the first part of this line, and has *up to* instead of *within*, to read *fill the measure up to the brim;*.

kittamawkasu,[63] kittamawkasu[64] entetwan![65]
take[66] pity on[67] us,[68] take[69] pity on[70] us[71] I say![72]

[63]*kittamakesew,* 5; *kittamau ke sue,* 10; NP 12; *kittamawkasu!* 13. [64]*kittamakesew* 5; *kittamaukesue,* 10; *kittamamawkasu* 13. [65]*Entetwan!* 9, 12. [66]*Take* 7, 8. [67]*of* I. [68]*us.* 8; NP 12. [69]*Take* 8. [70]*of* I. [71]*us,* I, 10, U. [72]*say!--* I; *say.* 7, 8.

warthow[73] enpechepemiskian[74] kedar[75] awewapemow;[76]
We[77] paddle[78] a long[79] way to see[80] you,[81]

[73] wathu (or wathei) 10; watheu 13. [74] enpechepemiskan 5; empechepemeskian 10, 13; empechepemiskian 12. [75] kether; 5. [76] awapemow (NP) 5; 10 has kedaraw wemepemou (NP). [77] we I, 5, 10. [78] come I. [79] Long I. [80] See I. [81] you; 8, U; You, 9.

pesquawemistacush[82] ennatum ut nema enpethtum;[83]
the french sends for us but we do not here,[84]

[82] pisquamistegush 5; pisquam wemistacush 10; pesquamwemistacush 13. [83] enpethtum, 5; impethtum (NP) 10; NP 12. [84] The English version appears only in I.

ensackehow[85] wemoustacushwuck[86]
we[87] love[88] the English,[89]

[85] insackehou 10. [86] wemistegushwuck, 5. [87] We 12. [88] Love I; 10 has we love, we love. [89] English; 5, 7, 13; English: 8; English. U.

cawickamawkasue[90] chestenmo,[91] amuskowwack[92] enkokotawan[93]
let[94] us trade[95] good black tobacco[96] (i.e.[97] Brazile[98]
tobacco)[99] moist and[100] hard twisted,[101]

[90] kawickamamaukasue 10; cawickamamawkasue 13. [91] chestinmou 10; chestenmow 13. [92] amiskowuck 5; awushouwuck 10. [93] enkokatawan 5; inkokotawan 10. [94] Let U. [95] I has give us instead of let us trade. [96] tobacco, U. [97] 5 and U lack the parenthesized portion; (i.e 7, 8; (ie) 10; (ie 12. [98] Brazil 9, 10; brazile 13. [99] I has good (braz$^{l}$. tobacco)

black tobacco,; tobacco (no parenthesis) 7, 8; Tobacco, 10. [100]& I. [101]twisted; 7, 8, U.

mothta[102] enquapanow[103] pettaw[104]
let[105] us see it[106] before opened;[107]

[102]motha 5; mothla 12. [103]enquapamow 5; enquappanow 9, 12; inquapanou 10. [104]NP 5, 10, 13; pettau (NP) 10. [105]Let I. [106]itt I. [107]op'n'd,-- I; opened, 5; opened. 7, 8, U.

kittamawkasu,[108] kittamawkasu[109] entetwan![110]
take[111] pity on[112] us,[113] take[114] pity on[115] us[116] I say![117]

[108]kittamakesew, 5; kittamaukesue, 10; NP 12; kittamawkasu! 13. [109]kittamakesew 5; kittamaukesue, 10; Kittamawkasu 12; kittamawkakasu, 13. [110]Entetwan! 9, 12. [111]Take 7, 8, U. [112]of I. [113]us; U. [114]Take 7, 8. [115]of I. [116]us, 10, U. [117]say!-- I; say. 7, 8.

mathattan[118] keperskasiggan;[119]
The[120] guns[121] are bad,[122]

[118]mathattin 5; Mathattan 9, 12; mathatan 10, 13. [119]kepaskasiggan, 5; kipperskashiggan (NP) 10; kepershasiggan (NP) 13. [120]the I, 5, 10. [121]Guns I, 10, 12. [122]bad; 8, 12; NP 10.

cawnahawack,[123] cawnahawack,[124] cawnahawack[125] parskasiggan[126] enkokotaway[127]
let[128] us trade light[129] guns,[130] small in the hand[131] and[132] well shaped[133]

[123]NP 5; this word occurs at the end of a page in 10 --

the text has *caurahawack* (NP), the catchword under it *caunahawack* (NP). [124]NP 5; *caunahawack,* 10, 13. [125]word lacking 5, 12; *cawnahawack,* 9; *caunahawack,* 10. [126]*paskasiggan,* 5; *paskersiggan* 10; *parshasiggan* 13. [127]*inkocotaway* 10. [128]*Let* I. [129]*Light* I. [130]NP I, 5; *Guns* (NP) 10. [131]*hand,* I, 7, 10, U. [132]*&* 12. [133]*shap'd,* I; *shaped,* 5, 10, U.

cowmescowwack[134] pewapisk,[135] anema wecuttamescowatin[136]
peponuck[137]
with locks[138] that will not freeze in the winter,[139]

[134]*caumiscouac* 10. [135]NP 5, 10. [136]*necutamiskoutin* 10. [137]*peponuck,* 5; *pepanock* 9, 12. [138]*Locks* I. [139]*winter;* 5, 8; *Winter* (NP) 10.

nishto[140] cawmisquack[141] spickannagun[142]
and[143] red[144] gun[145] casses;[146]

[140]*mishto,* 10. [141]*coumisquac* 10. [142]*spiccannagun* (NP) 10; *spickannagun;* 12. [143]*&* 12. [144]*Red* I. [145]*Gun* 10; *Gun-* 12. [146]*casses,* I.; *cases;* 5, 12, 13; *casses:* 8; *cases.* 10, U; I adds: (for if a gun is bad, a fine case often putts it of, being great admirers of Differ$^{t}$ Colour's)--.

camethoshashat[147] wappostemmo[148] wiskeneck[149] cuthewow[150]
harrawack[151] tipahow;[152]
let[153] the young[154] men[155] have more than measure,[156] role[157]
tobacco[158] cheap,[159]

[147]*cawmethoshashit; cawmethoshashit* 5; *camethoshashet* 10. [148]*wappe stemmow* 5; *nappestemmo* 10. [149]*wiskeneck,* 5;

*wiskenec* 10. [150]*cathewow* 10. [151]*huthawack* 5; *harrawuc* 10; *hathawack* 12; *harrawuck* 13. [152]*tippahaw,* 5; *tippahou* (NP) 10; NP 12. [153]*Let* I, 10, U. [154]*Young* 12. [155]*Men* 10. [156]I lacks *more than measure,;* *measure;* 7, 8, 12; *Measure* (NP) 10; NP U. [157]*Roll* I; *roll* 10; *of* U (thus reading *more than measure of tobacco;*). [158]*Tobacco;* 10; *tobacco;* U. [159]*cheap;* 7, 8, 12; NP 10, U.

nishto nahowock[160] arskeck[161] kakespuckow[162] enkokokatawan[163]
kettles[164] thick,[165] high[166] for the shape[167] and[168] size,[169]
strong ears,[170] and the bale[171] to lap[172] upon[173] the sides;[174]

[160]*nahawoc* 10; *nahowuck* 12. [161]*arskick* 5; *arskeek* 13. [162]*kakespuckow,* 9; *kakespuc kou* 10; *ka kespuckow* 12. [163]*enkokokatawan,* 5; *inkocotawan* 10. [164]*Ketles* I; *kettles,* 10, U. [165]NP 5. [166]*and high,* 5; *and high.* U (which lacks the rest of the section). [167]*shape,* I; *and well shaped;* 5; *shape.* 9. [168]*&* 12. [169]*size,* 8, 9; 5 lacks *and size,*. [170]*Ears,* I; NP 5, 9, 12. [171]*Ba$^{i}$le* I. [172]*Lap* I. [173]*Just upon* I; *on* 10. [174]*side,--* I; *sides,* 5, 10; *sides:* 8, U.

hurrawack[175] tipahow[176] mantuiggan,[177]
give[178] us good[179] measure[180] in[181] cloth,[182]

[175]*huthawack* 5; *harrawac* 10. [176]*tippahaw* (comma erased) 5; *tippahou* 10; *tipahow,* 12. [177]*mantuihiggan* (NP) 10; NP 12. [178]*Give* I, 10, 13, U. [179]*Good* I. [180]*measure,* I. [181]*of* U. [182]*cloth,--* I; *cloth;* 5, 7, 8, U.

mothta keash tippahigan[183] enkowopomow[184]
let[185] us see the old measure;[186]

[183]*tipahigan* 12. [184]*enkawapemow,* 5; *encowappowou* 10; *enko wopomow* 12. [185]*Let* I. [186]*measures,* I; *measurs,* 5; *measure,* 10, 13; *measure:* 12.

kepathtemuck[187] nauma![188]
do[189] you mind me?[190]

[187]*kepathtewuc* 10. [188]NP 5; *nawme* (NP) 10; *nawma* (NP) 12. [189]*Do* I. [190]*me?,* I (the MS has ;; printed in Isham 1949 as *!,*); *me,* 5, 12; *me.* 7, 8; *me!* 10.

kesackehow[191] wiskenick[192]
the[193] young[194] men[195] loves[196] you[197] by coming[198] to see you;[199]

[191]*kesackehow;* 5; *keshaskehow* 10; *kesackehow,* 12. [192]*wiskeneck* 5; *wiskinic* 10. [193]*The* 7, 8, 10, U. [194]*Young* 12. [195]*Men* 8, 9, 10. [196]*Loves* I; *love* 10, 12. [197]*you,* U. [198]*comming* I; *coming so far* U. [199]*you,* I; *you.* 7, 8; *you:* 10.

kittamawkasu,[200] kittamawkasu[201] entetwan![202]
take[203] pity, take[204] pity[205] I say![206]

[200]*kittamakesew,* 5; *kittamau kesuc,* 10; NP 12; *kittamamawkasu!* 13. [201]*kittamakesew* 5; *kittamaukesuc,* 10. [202]*Entetwan* (NP) 12; NP 13. [203]*Take* 7, 8. [204]*Take* 7, 8. [205]*pity,* 10, U. [206]*say!--* I; *say,* 7; *say:* 8; NP 12; *say;* U.

cawmathushashit[207] ionish[208] cuttiewow[209]
and[210] give them good goods;[211]

[207]*cawmethoshashit* 5; *cawmathshashit* 9, 12; *caumathusishit*

10; *cawmathusishit* 13. [208]*inoish* 9, 12. [209]*cuttaiewow,* 5; *cuttiewou* 10. [210]*And* 8. [211]I has *give them good,*; *goods,* 5; *goods.* 7; *goods:* 8, 13; *Goods,* 10.

awecuttamarewenawkoshit[212] awewusheshot[213]
they[214] love[215] to dress[216] and[217] be fine;[218]

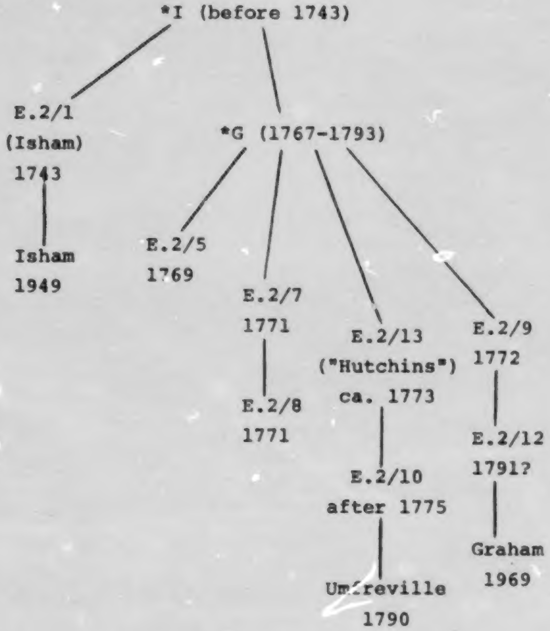
[212]*awecuttamathewenawkoshit* 5; *awecuttamarewenawkoshit;* 12 (misprinted as *awettamarewenawkoshit;* in Graham 1969).
[213]*awethushehot* 5; *awewusheshot,* 10; *awewusheshot;* 13; in 12 the word occurs at the end of a page -- Graham wrote awenush-//awewusheshot (printed solid in Graham 1969).
[214]*They* 7. [215]*Love* I; *like* U. [216]*Dress* I; *dress,* 8.
[217]*&* 12. [218]*fine,* I, 13; *fine:* 8, 10, 12.

keneshetutin nauma?[219]
do[220] you understand me?[221]

[219]*nauma.* 5; *nawma.* 12. [220]*Do* U. [221]*me!--* I; *me.* 5, 7, 8, 12.

The Indian Leader's Speech was originally written by James Isham. Not only does it appear first in a manuscript bearing his name, but the Cree version (although it appears only in Graham's manuscripts) contains a number of spellings unique to Isham, e.g. *kedar* 'you' and *pethko* 'gunpowder'. Although Isham spent his entire career at York Factory and Churchill, he occasionally used words from the now extinct *r*-dialect that was spoken at 'the bottom of the Bay' between Rupert House and Moose Factory, such as *ha ra ca naw* 'bread' (cf. Gorst's *arakana* and the place name *Harricanaw River*). Graham, on the other hand, used Woods Cree forms consistently (he gives *hethekanaw* 'biscuit, bread' in his Cree vocabulary). Most of the Indian Leader's Speech is in Woods Cree, but there are three words in the *r*-dialect: *ketuskerogan* 'your servant', *harrawack* and *hurrawack* 'more than', and *awecuttamarewenawkoshit* 'that he may appear fine'. In E.2/5 the Cree text is more thoroughly revised than in any of the others; Graham has changed 'your servant' to *(t)atawan mistegush* 'the trading Englishman', and in the other words changed *r* to the Woods Cree *th*. In E.2/13 (and its derivative E.2/10) he changed *ketuskerogan* to *ke tuskethogan*, and in E.2/12 he wrote *hathawack* once for *harrawack*, but the remaining *r*-dialect words remain as anomalies in his manuscripts.

This is not a copy of an actual speech (the Cree is often ungrammatical), but a composite of the many speeches Isham would have heard during his career. There were probably two more versions than now exist: the original copy, with the Cree text, of Isham's *Observations*, and another copy in the rough notes on which Graham based the various versions of his own *Observations*; this latter copy Graham must have revised from time to time, since no two copies have the same text. It is quite easy to construct a family tree of the manuscripts based on the variations in the speech; words 12, 40, 41, 47, 118, and 207 are sufficient to show the relationships of the manuscripts containing the Cree text (i.e. E.2/5, 9, 10, 12 and 13), and there is plenty of evidence to show how the remaining copies fit in. This stemma is based only on the Indian Leader's Speech; it should also be valid for the remainders of the manuscripts, but I have not attempted to check this. *I and *G represent the missing Isham and Graham manuscripts:


*I (before 1743)
E.2/1
(Isham)
1743
Isham
1949
*G (1767-1793)
E.2/5
1769
E.2/7
1771
E.2/8
1771
E.2/13
("Hutchins")
ca. 1773
E.2/10
after 1775
Umfreville
1790
E.2/9
1772
E.2/12
1791?
Graham
1969

## NOTES

[1] This is a revised version of the paper read at the Seventh Algonquian Conference, Niagara-on-the-Lake, October 31, 1975. After consulting the original manuscripts in the Hudson's Bay Company Archives (published here by kind permission of the Company) I have changed some of the opinions I formed based on my study of the printed versions.

I would like to thank Roy Wright for his comments on the earlier version, J. K. Chambers for help with the Siouan materials, and Mrs. Shirlee Smith for her courteous assistance at the archives in Winnipeg.

[2] The only known copy of the Blackfoot vocabulary is in E.2/12, a copy Graham made ca. 1791, but he likely obtained it from the Indians he met in 1772 at York Factory. Twenty-two English glosses are matched by thirty words in various languages. The first entry, 'A Knife', is translated as *Estewan-Wath (or) Mein-me-amungo* (sic, misprinted in Graham 1969), i.e. Blackfoot *Estewan* (modern /isttoan/), Atsina *Wath* (modern /wóoθo/), Assiniboine *Mein*, and *me-amungo* (language?). The other likely Atsina word is *Cattee* 'gun' (modern /kocíyo/). Graham gives 'tobacco' as *Pistachkansheshawan*: the first half is Blackfoot /pistaxkan/, the second apparently Arapaho /s̲ííisowoo/ (not Atsina, which would have been /cíisóówoo/, modern /cíiθóówoo/). The other Assiniboine words are *Tanjut* 'ball', *Chekee*, 'kettle', *Meneago* 'brandy', *Chonga* 'dog', *Champa* 'beaver' and *Sketonga* 'horse'; I have not been able

to identify the language of *Makquee* 'wolf' and *Masp* 'hatchet' (and *me-amungo* 'knife', above).

[3] Umfreville's own observations (1790:146) would place him near Dawson Creek, B.C. He was actually near Frenchman Butte, Saskatchewan, at Section 4, Township 53, Range 25 (West of the third meridian), according to J. B. Tyrrell (quoted in Douglas 1929:6).

[4] This strange word seems to be a compound of Cree /(o)kiskiman-/ (cf. *okeškemani·ʔsiwa 'kingfisher') and /-i·htiko·w/ (*wi·nteko·wa 'cannibal monster'); the spelling -*tht*- represents the [θt] allophone of /ht/ often found in the early fur trade vocabularies of Woods Cree (see my paper "Proto-Algonquian *sk in Woods Cree", to appear in E.-D. Cook and J. D. Kaye, eds., *Linguistic studies of native Canada*).

[5] In E.2/12, one of the versions of his *Observations* that Graham copied out himself, the Indian words are uniformly written without divisions. This must also have been true of *G, the lost manuscript that was the parent of part of the text of all the extant copies of Graham's *Observations*, since all the versions of the Indian Leader's Speech (see Appendix) are spelled this way. Manuscripts E.2/4, 6, 7 and 8 contain no Cree at all, but E.2/5, 9, 10 and 13 mark syllables by spacing, hyphens, apostrophes, commas, and various combinations of these, in the main entries (not in Graham's writing); Graham's own later additions are written solid.

[6] Several of Umfreville's words do not look at all like their

modern counterparts, e.g. Atsina *Nun-nec-so-on* 'An Eye', modern /besíísеʔ/ (Mi35d), and Sarcee *Me-cut-chin-er* 'Tobacco', modern /nádìsgòni/. The printed version of Graham's *Observations* has Sarcee *Senowwok* 'eye' where the manuscript reads *Senowwoh*, and *Hey* 'dog' where Graham wrote *Tley*; there are also errors elsewhere in the Indian portions of Graham 1969. In Umfreville's vocabulary, the Cree, Assiniboine and Blackfoot words for 'A File' are correct, but the Sarcee entry, *Til-te-the-te-co-sey*, contains the word for 'gun' (*Til-te-tha*), and the Atsina word, *Oh-er-er*, seems to be the same as Peter Fidler's *O hi oh* 'flint' (I hope to publish the Fidler vocabulary in the near future; it is in the HBC Archives, B.34/a/1). It appears that Umfreville somehow entered the Atsina and Sarcee words for 'flint' in the line for 'file'; it is likely that his published vocabulary was compiled from somewhat longer lists of the individual languages.

[7] The Hudson's Bay Company received two letters from Umfreville in early 1789, the first of which was dated 'Canterbury place Lambeth 23$^{d}$ Jan$^{y}$ 89' (HBC Archives, agenda book A.4/4, 56$^{v}$), while they also received at least a dozen letters addressed from Edinburgh by Andrew Graham during the year.

[8] 'When M$^{r}$ Colen left England last he took with him a Time Peice and also a large bound Manuscript Book by M$^{r}$ And$^{w}$ Graham containing remarks during his Residence at the Bay -- both these articles We desire may be return'd by Captain Richards the latter We did not know M$^{r}$ Colen had taken with him till lately.' (paragraph 16 of the Committee's letter to Mess$^{rs}$

Joseph Colen & Council, York Factory, 31 May 1797; HBC Archives A.6/16, $41^{v}$-$44^{v}$).

[9] What little is known about Umfreville's life outside of his Hudson's Bay Company career is given in Wallace's introduction to Umfreville 1954. Williams provides a biography of Andrew Graham in Appendix A of Graham 1969. No record has been found of either man's place and date of birth.

[10] The entry is given in Grant, ed., 1922:294, as 'Graham, Andrew, factor for the Hon. Hudson Bay Company, London, and Patricia, d(aughter) of deceast James Sherer, merchant, both in Old Grayfreir's p(arish), 6 May 1770.'

[11] Wyld (1920:298-300) shows that the loss of *r* had started by the middle of the fifteenth century in Essex and Suffolk; by the middle of the sixteenth century it was typical of lower-class London speech, and by the middle of the seventeenth century had been adopted by the upper class as well.

[12] For example, on pp. 189-192 (= 1954:98-99) Umfreville writes of the Plains Indians' religion, system of numbers, and calendar, in very similar words to those he used when describing the Cree around York Factory on pp. 42-43, 54-55 (= 1954:22, 27-28).

[13] See Wallace's introduction, Umfreville 1954:xiv-xv, and note 7 above.

[14] This was normal business practice: the same thing happened to Henry Kelsey on the two occasions he was captured by the French more than eighty years earlier. Even the Royal Navy

laid off its officers on half pay when they were taken prisoner.

[15] Two letters dated October 10, 1782, from the prisoners were read at the Committee meeting of November 6; a reply was sent saying 'the Committee will apply for their Protection, on their assurance of continuing in the Company's Service.' A week later another letter, dated November 8, was read (HBC Archives, rough Minute Book 1782-84, A.1/142, 52-53), and the Company wrote the Admiralty to ask that the prisoners not be impressed on their arrival in England. At their meeting of January 22, 1783, the Committee read a letter from the released prisoners 'on board the Diligent Man of War at Spithead with$^{t}$ date'; they also received a letter from Matthew Colling dated January 18 on board the *Diligent* (HBC Archives, A.1/142, 65).

[16] The London Committee read a letter 'from Edw$^{d}$ Umfreville dated London 10 March' at their March 12, 1783, meeting; they also received letters from Samuel Hearne and a number of other employees who had been released by the French, no doubt all asking for back pay. On April 2 the Committee ordered its Secretary to write 'to the several Persons late Servants to the Company, who have applied for Wages to be continued them for the present Year, that their Request cannot be Complied with & to desire their immediate answer whether or not they are willing to Return to their former Stations' (HBC Archives, A.1/142, 81, 87). Hearne accepted the offer, but Umfreville did not.

[17] This is the best documented part of Umfreville's life. The journal he kept while exploring west of Lake Nipigon has survived (printed by R. Douglas in 1929), and the Frobishers refer to him in a letter and memorial to Frederick Haldimand on October 4, 1784 (PAC, Q.24-2, 405, 409, printed in Canada, Public Archives 1891:48-52).

[18] The copy of Hearne's journal that Umfreville quotes is likely, but not necessarily, one in the Graham manuscripts. There were many copies in circulation in the 1780's; Hearne (1795:xviii) specifically states that he is publishing his journal because he is 'well assured that several learned and curious gentlemen are in possession of manuscript copies' containing conflicting information.

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# COLOR TERMS IN NARRAGANSETT

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Color space has often been considered as a prime example of the arbitrariness of the fit between physical phenomena and the terms chosen by any given language to refer to these same phenomena. The discussion by Gleason (1955:4) on the varying ways English, Bassa, and Shona divide up the spectrum is typical of this approach to the question. A quite different view of color space and color terminology is advanced by Berlin and Kay in their book, *Basic color terms*, in which they argue for the existence of just eleven universal basic color terms acquired by languages in a fixed evolutionary order:

Stage I: WHITE *and* BLACK
Stage II: RED
Stage III: GREEN *or* YELLOW
Stage IV: GREEN *and* YELLOW
Stage V: BLUE
Stage VI: BROWN
Stage VII: PURPLE *and/or* PINK *and/or* ORGANGE *and/or* GREY (any number in any order)

In the spate of criticism and comment provoked by this book, two studies are of special interest. Foremost is 'Color term salience' by D. Hays *et al.*, some of the conclusions of which are independently supported by E. Heider in her article, 'Universals in color naming and memory'.

In their paper, 'Color term salience' (hereafter referred to as CTS), Hays and his associates conclude (1972:1120) that, in spite of their agreement with many of the criticisms of Berlin and Kay's book, they are persuaded that its main structure is correct. CTS (1972:1109-13) adduces four new types of evidence in support of this conclusion.

(1) Color vocabulary size and cultural complexity: Few focal color terms are found in usually simple societies, and more focal color terms in more complex societies, a fact they maintain is not accounted for by the hypothesis of joint diffusion.

(2) Expression length and salience: The more salient a focal color term, the shorter its expression form on the average.

(3) Frequency of use and salience: The more salient a focal color term, the more often it is used in a culture that possesses it.

(4) Ethnographic frequency and salience: The more salient a focal color term, the more frequently ethnographers are likely to use it when discussing native ideas about color. Also, the more salient a term, the more likely it is to appear in an ethnography that mentions few colors in such discussion.

Two related concepts are central to this discussion, *focal color term* (which replaces Berlin and Kay's troublesome *basic color term*) and *salience*. By a *focal color term* is meant 'a monolexemic color term which is substantially equivalent to one of the following eleven English color terms: black, white, red, yellow, green, blue, brown, pink, orange, purple, and grey' (CTS 1972:1108). Each such term is defined by a focal point of hue and brightness on the Munsell color chart (CTS 1972:1109). By *salience* of colors is meant 'those colors which are notable for their prominence and conspicuousness, and thus attract attention. Salient colors 'leap out'' (CTS 1972:1111). All focal color terms are salient in this sense, with the more widely distributed terms being more salient than the others (CTS 1972:1111-13, 1120).

In this paper, I will examine in the light of the evidence of the Narragansett materials the argument of CTS that there is a relationship between ethnographic frequency and salience. Since it is maintained that this relationship can be determined from written records alone, in the absence of native informants (CTS 1972:1113), Narragansett provides a good test of the validity of this contention. Less directly, the Narragansett evidence also tests the other three conclusions of CTS. In addition, the following discussion of Narragansett color terms is of more general interest due to the fact that Algonquian languages are seldom adequately treated in the various discussions of color terminology. CTS briefly refers to four Algonquian languages in one of its tables (CTS 1972:1114-17); neither Heider nor Berlin and Kay mention any at all. I have attempted to be as complete as possible, including in this discussion any and all terms that may reasonably be considered Narragansett color terms in the light of information available to me. I have also included both Proto-Algonquian and Natick color terms where I have been able to determine them.

1. **mówi** 'black' (191); **mowáshuck** 'iron' (42); **mowêsu** 'blacke, or swarfish' (52); **moattôqus** 'a blacke wolfe' (103); **móaskuq** 'black snake' (105)
   -cf. Natick **m8i** '(it is) black, dark colored' (Tr03.65), PA ***mo:w-** 'soil' (B125.139)

2. **sucki** 'black' (52); **súcki** 'blacke' (155); **súcki** 'blacke' (191); **suckêsu** 'blacke, or swarfish' (52); **suckáutacone** 'a cole-blacke man' (52); **suckaũhock** 'black money' (115); **suckáwhock** 'blacke money' (115); **suckáuhock** 'black [money]' (155); **suckaúhock** 'the blacke money' (155); **nausakésachick** 'the blacke money' (155)[1]; **suckaùanaûsuck** 'the blacke shells' (156); **suckauaskéesaquash** 'the blacke eyes' (156); **súckinuit** 'black, or blackish [cloth]' (160); Stiles **suckequoq** 'bass' (Co73.9); Gatschet **súckenak** (a plural form) 'black, colored persons' (Ga73.14)

3. **métewis** 'black earth' (192); **metewêmesick** '...an Indian towne...' (192)

4. **wómpi** 'white' (191); **wompésu** 'white' (52); **wómpimish** 'a chestnut tree' (95); **wómpimineash** 'chestnutts' (95); **wompiscannémeneash** 'white seed-corne' (98); **wompmissaûnd** 'a chestnut canow' (108); **wòmpans** 'white money' (115); **wompam** 'white [money]' (155); **wauômpeg**, or **wauompésichick-mêsim** 'give me white' (156); **wómpinuit** 'white cloath' (160); **wompeqũayi** 'cloth inclining to white' (160); Stiles **waumpmunch** 'chestnut' (Co73.10); Gatschet **wámpūm** 'money; currency used as money' (Ga73.14)
   -cf. Natick **wómpi** 'white' (Tr03.195), PA ***wa:pi** 'white' (B146.104)

5. **msqùi** 'red' (191); **míshquockuk** 'a red copper kettle' (15); **mishquáwtuck** 'the cedar tree' (96); **mishquáshim** 'a red fox' (103); **mishquammauquock** 'red fish, salmon' (113); **mishquòck** 'red earth' (133); **míshquock** '...Adam, or red earth... (134); **míshquinuit** 'red cloth' (160); **míshquock** 'red earth' (192); **mishquésu kunúkkeke** 'your otter is reddish' (162); Stiles **mishquāsmps** 'fox' (Co73.10)
   -cf. Natick **mŭsqui**, **mĭshqui**, **msqui**, and **-que** '(it is) red'(Tr03.71), PA ***meskw-** 'red (root)' (B146.120)

6. wunnàm 'their red painting...' (191)
   -cf. PA *welamana 'red ochre, vermilion' (He71.247)

7. wesaûi 'yellow' (191)
   -cf. Natick weesóe 'yellow' (Tr03.186), PA *wi:sa:wi 'yellow' (Go65.220)

8. askáski 'greene' (191)
   -cf. Natick ashkoshqui, -ki; oshkoski '(it is) green' (Tr03.15)

9. peshaũi 'blew' (191); peshaûiuash 'violet leaves' (97)
   -cf. Natick 8nói, 8nóe 'blue' (Tr03.112)

10. péquawus 'a gray fox' (103)
    -cf. Natick wompishocki 'gray' (Tr03.195)

Of the ten color terms found in the Narragansett materials, one of them, term 6, does not fit the definition of a focal color term and must be eliminated.

Several other terms are questionable. In spite of the numerous examples for term 2, Trumbull (1903:226) rejects the interpretation of Williams, stating that

> ...strictly speaking, súcki was dark colored and not black. The dark purple shells from which the more valuable peag was made, and the dark peag itself --- blue, purple, or violet --- were named from their color suckaúhock.

Term 3 is glossed with a query by Trumbull (1903:56) as 'plumbago or graphite', thus suggesting that it may not be or contain a color term at all. Term 10 is suspect for the same reason.

Term 9, glossed by Williams as both 'blue' and 'violet', may indicate only that the term BLUE in Narragansett also includes some shades of purple. Trumbull (1903:112) questions the forms cited by Williams, pointing out that the Narragansett forms are apparently related to the Natick form uppeshau 'flower'. Lending support to Williams, however, are the Natick forms peshai 'blue' and peshânnoquat 'blue colour' quoted by Cotton (1830:168). Given this conflicting evidence, term 9 must be considered questionable.

If we eliminate from our list, in addition to term 6, those terms which are suspect in some way, i.e., terms 2, 3, 9, and 10, we are left with evidence for the existence of Narragansett color terms for BLACK, WHITE, RED, YELLOW, and GREEN. Most striking is the fact that the color terms found in the Narragansett materials are just those predicted by Berlin and Kay, a finding which supports the contention of CTS that there is a relationship between ethnographic frequency and salience. In addition, since there are far more examples of BLACK, WHITE, and RED in the Narragansett corpus than of the other terms, it can be argued that the salience of the focal color term is related to its frequency of use, with the earlier terms in Berlin and Kay's evolutionary sequence having higher frequencies than the later terms. Further, according to this evolutionary sequence, Narragansett would be a Stage IV language, lending some support to the contention of Berlin and Kay and CTS that there is a relationship between color vocabulary size and cultural complexity.

Concerning the question of expression length and salience, it is sometimes stated in the discussion of focal color terms that the more widely distributed terms are shorter in length than the rarer ones (CTS 1972:1111; Heider 1972:14). In this connection, Heider talks of letters and CTS refers to phonemes. Neither makes very clear what is meant precisely by 'letter' or 'phoneme'. Neither study, moreover, discusses the phonology of any of the languages used in any detail. Further, there is little discussion of the problems involved in the determination of length regardless of the unit of analysis selected.[2] Until more attention is paid in the literature to considerations of this nature, any conclusions concerning expression length and salience are best viewed with some skepticism.

The Narragansett evidence thus seems to generally support the conclusions of Berlin and Kay and of CTS, with the one exception noted. Although this analysis of the Narragansett materials is a tentative one, there is reason to believe that some of the attributes of focal colors, including their linguistic codability across languages, are universals probably derived from underlying factors that have to do with the physiology of primate color vision (Heider 1972:20). In the case at hand, of course, before making any claims of this nature, we need more information on the color terms in languages closely related to Narragansett, as well as Proto-Algonquian reconstructions for as many of the eleven focal color terms as possible. If and when this information becomes available, a clearer picture of the Narragansett situation and its wider implications will hopefully emerge.

## NOTES

[1] Ives Goddard has pointed out to me that this is most likely the changed form; cf. Unami sə́ksu 'he is black', Munsee nsə́ksəw 'he is black' and ne·skə́si·t 'one who is black'.

[2] See Durbin (1971:271 note) for a brief discussion of some of these problems.

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# CHRISTIAN HOLIDAYS IMPORTANT TO THE WABANAKI

Nicholas N. Smith
Ogdensburg Public Library

The influence of missionaries and Christianity on the native Americans has been largely ignored and overlooked by anthropologists. Many, like Frank Speck, deplored and detested the missionaries for introducing changes to the Indian's way of life. Much of importance has been neglected in primary acculturation between the American Indian and the Europeans. Among these are the Christian holidays as celebrated by the Wabanaki.

The 17th century missionaries who came to save the souls of the North American natives underestimated the development of the Indians' religion. The Indian was accepted only as a lowly savage. His complete way of life must change. He would have to become a typical member of 17th century European agrarian society as evidence that he had been converted.

The Wabanaki were deeply spiritual people. In many ways their religion paralleled Christianity. They probably understood far more about Christianity than they admitted to the newcomers. The natives believed in a Supreme Being. They regularly gave thanks to a Supreme Being, for the problems of each day were many. The dead went above to the Milky Way where they would enjoy excellent hunting and never again would know the pangs of hunger. They asked their Supreme Being to spare their life during crisis situations, which were many. Both bad and good spirits were lurking everywhere. If they were offended, they were ready to wreak vengeance on the unlucky one. The Wabanaki religion was one based on fear and vengeance.

Religion was of such great importance to the Wabanaki, they would take a keen interest in observing the European religious rituals. In 1608 Lescarbot and Pountrincourt began teaching the Port Royal Indians Christianity. Two years later Pountrincourt brought the secular priest Messire Jessé Fleché to Port Royal. Soon, on St. John's Day, he baptized 24 Indians and their Chief, Membertou. In 1611 Biard and Masse, Jesuits, arrived at Port Royal where they established their headquarters. They made long journeys going as far as the Kenebec River in Maine. In 1613 Du Thet, Quentin and Lalemant arrived. Du Thet was killed when the Virginian Argall attacked the mission established on Mt. Desert Island. In 1619 Recollects from Aquitaine established a mission on the St. John River. In 10 years the endeavor to save the Indians' souls increased from 2 lay teachers to the establishment of missions at Port Royal, Mt. Desert Island, and the St. John River, the missionaries making long trips to convert the pagans. (Thwaites 1902: Vol. I-13; Shea 1855: 130-133).

Before 1700 it was claimed that all the Micmac from Gaspe to Nova Scotia had been converted, and additional missions were established at Norridgewolk and Old Town. More missionaries arrived and a mission was established at Meductic on the St. John River. It was said of Father Charles Germain, a Jesuit who came to the New World in 1738, served at Old Town and Meductic and died at St. Francis in 1779:

> "He may be regarded as the last of the old missioners to the Indians of Maine, who planted the faith so firmly in the hearts of that Algonquian race that neither privation of priest and altar, nor all the allurements of prosperous and pretentious error could lure them from it." (Shea 1886: Vol. I-605).

During the years preceding the Revolution it was difficult for Catholic missionaries in Maine. After the fall of Quebec in 1763 the French missionaries in Maine withdrew to Canada. Those at the French mission Indian villages in Canada tried to persuade all the Indians in Maine to emigrate to their settlements. Most of those on the Kennebec moved to St. Francis de Sales.

Massachusetts asked the Wabanaki to take the side of the Americans during the Revolution. The Indians agreed to back the Americans, but asked for a "Black Gown" or French priest. The Massachusetts General Court declared themselves ready to get a French priest, but did not know where to get one. (Shea 1885: 155).

A Protestant minister was offered. Chief Orono refused the offer saying, "We know our religion and love it: we know nothing of you or yours." (Shea 1855: 156).

In 1774 another request for a priest was made with no success so a group went to Providence, R.I., where a part of the French fleet was in port. This attempt to obtain a French priest was also unsuccessful.

On May 31, 1791 Col. Allen wrote to Bishop Carroll in a further attempt to obtain a priest:

> "They are a very exemplary people, consistent with their customs and manners, as are to be met with, zealous and tenacious of the rites of the Church and strictly moral, cautious of misbehaving in point of religion. Though rude and uncultivated in many other matters, they are truly cultivated in this, and it was always observed by the French gentlemen of the clergy, whom we were favored with during the war, that they never saw a more respectable collection in France, and excepting the Cathedrals and some particular place of worship, their performance, chants, etc., in Latin, were in most instances superior to any. I have been myself charmed with them when shut up in the woods. And though of different sentiment, believe them truly to be Christians, meriting the peculiar blessings of the Deity. They teach their children when able to lisp a word, the service, and as they grow up, become in a manner innate, this owing to the assiduity of the French missionaries, much to their honor. (Shea 1888: Vol. II-393).

The letter suggests that occasionally a missionary from Canada visited the Maine Indian villages during the Revolution. The missionaries' hard work had results, and after the Revolution, priests were assigned to the Maine Indian missions, which are still served today.

The Wabanaki were devoted to their new faith which had far more hope to offer than did their pagan religion. Their harsh life had forced them into a disciplined religion which they now transposed to regulations of Christian life. The work of converting the people had been finished years before;

now the job was to fulfill the regular Christian routines.

One of the major problems of the early missionaries was the language barrier. Many attempted a dictionary and then translated parts of the Prayer Book, hymns, or Bible. Finally in 1857 Vetromile published a Prayer Book in several Wabanaki dialects. This was based on the work of Romagne and Demillier. In 1950 some of the older Indians still considered these Prayer Books prized possessions and took them to the grave. Most of the older people knew many of the hymns and prayers in the Vetromile book by heart. At Pleasant Point, Maine, an old blind Passamaquoddy woman was teaching the children the hymns and prayers from it. The discipline of their former religion had taught them how necessary it was to pray daily and keep the holy days.

Some of the basics in the acculturation process were interpreted through Christianity such as the names for the days of the week and the months of the year. All lists of the names of the days of the week checked were similar, which suggests that the weeks and the days of the week were not divisions in time until the arrival of the Europeans. No two lists of the months were exactly alike. (Alger 1885: 246; Laurent 1884: 19; Speck 1940: 265).

The most important day of the week became Sunday or "Sun-te." Monday became the "day after Sunday" or "Ki-sun-te." Friday, "Ske-he-ea-tukw" or "the day of the cross," reminded all of Good Friday. Saturday was "the day before Sunday" or "Ku-tu-a-sun-te." Even the term for one week incorporated Sunday, "Newkt-sun-te," or "between Sundays." The importance of the day for worshipping God was established from the beginning.

The importance of Christmas was introduced by a new name for the month of December, "Ni-pa-yum-mi-ham-ke-wi-ki-sos," or "Prayer-at-night moon."

The new schedule of days did not stop there, for the entire Christian calendar with many important new festivals was introduced. Each was introduced with its characteristic custom. Often there was a melding of Catholic and Wabanaki traditions for the festival celebrations.

Christmas or Ni-pa-yum-mi-ho-muk was not the holiday that we have made it. The birthday of Christ was notable for the midnight mass. Gift giving was probably unknown until fairly recent time. (Wallis 1957: 18). Candle light was the typical light for the people so it could not have the same romantic atmosphere that most of us feel when we enter a room illuminated entirely by the soft flickering tongues of flame. The real importance of Christmas was that it was the anniversary of the birth of their Savior.

It was a time to honor all those whose name was Mali or Noel. After the midnight mass there would be a procession to the homes of all who were named Noel or Mali. Usually the chief or head man led the procession. He made a cross of wood for the occasion. All those in the settlement would follow as they paraded to the various homes of those to be honored. Christmas hymns were sung, a special hymn was sung to the honored one, and the beads were said. The hymn sung to those named Noel was:

Ma-we-aw-mon-ege, Noel
Let us be with him, Noel.

The leader, usually the head man, would then say the beads. He was called "Nee-con-ee-me-ak," or prayer leader. Then the one honored would kiss the cross. If there were many to be honored in the settlement, the dignified ceremony might last several hours.

The Malecite of Woodstock, N.B., did not have a Christmas tree until the early 1930's. Presents consisting of oranges, nuts and candy filled children's socks. Gift giving was not common until the 1950's. At Tobique the custom of the Christmas tree and gift giving started about the same time as they did in Woodstock (Wallis 1957: 18). The St. Francis Abnaki children go from door to door with a bag and are given candy and fruit. The grownups display their affection by passing the youngsters around to be hugged and kissed, something not always appreciated by the children. In 1967 a celebration for children was started at St. Francis. The children make toys. On Christmas they form a procession and carry baskets with their gifts to the Shrine of Mary. They sing an old Indian hymn which in English translation is:

> Let us be glad because our Lord Jesus Christ was born.
> You accepted some of our ancestor's gifts;
> Please accept ours.

The radio and T.V. have been major factors in hastening the pace of acculturation of major holidays such as Christmas.

Epiphany, the Twelfth Day after Christmas, was known as Kinjamesak. In many parts of the world people still celebrate this data as the major festival of the Christmas season. It is symbolic of the time when the three wisemen presented their gifts to the new born Messiah.

The Wabanaki celebrated Kin-jam-es-win, "Day for a king," with a big feast. If anyone had a birthday about that date, he might celebrate it at this time. The feast was held in the council house. The men sat at one table and the women at another. If there was not enough room for everyone to eat at the same time, the men ate first and the women ate after they finished. For dessert there were two cakes, one for the men, and the other for the women. Sometimes the St. Francis made small individual cakes. The Malecite and Passamaquoddy put a bean or a button in each cake. The St. Francis put a pea in one of the men's cakes and a bean in one of the women's cakes. The man and woman who received the piece of cake with these items would become King and Queen reigning over the dance which followed. They wore paper crowns and sat on a platform above the others. They led the dancing which customarily lasted until daylight. This followed the French custom which was called "le jour des rois". (Albert 1969: 108). Dancing was the method of celebration which meant the most to the Wabanaki. It was always a great deal of fun to see who would get the button, bean, or pea, and most of the older folks still remember with delight these occasions. This celebration might be compared to the Mardi Gras. For most Wabanaki it was the last festivity before Lent. When the people were on a trapping economy, they returned to the villages for Christmas, remaining there for Kinjamesak and then returned to the bush for another six to eight weeks.

New Year's Day is generally considered a secular holiday rather than a religious holiday and no word list of Christian holidays included New Years. However, the following New Year's custom appears to be based on Christianity so it is included here. At Woodstock everyone would visit all the others in the community. Peter Paul described it: Anyone could start the visiting which usually began at 4 or 5 o'clock in the morning. The entire family would arise, including the children, and visit their neighbor. They would embrace, touch cheeks, both sides of the face, each member of the household, and then proceed to the next house. When the first family had left the second house and proceeded to the third, the first family visited would then prepare to visit and so the procedure would continue until everyone had visited and embraced everyone in the village. It all sounds rather idealistic. It was emphasized that it would be hard to carry a grudge against someone into the New Year after participating in this ritual.

Ash Wednesday was known to the Malecite as "Tupkw-un-ho-dine" or "ashes put on you time." It was a time for fasting. Most of the families were in the bush on their trap lines so nothing was done on a community basis. Families could put ashes on their foreheads and they could fast, at least for breakfast.

Palm Sunday has been notable for people carrying palms. In the northeast the missionaries substituted evergreen boughs for palms. The St. Francis term for Palm Sunday is "Sediak kalnomuk". (Laurent 1884: 56). Rasles also used the term "Sediak" (Rasles 1833: 89) in reference to Palm Sunday and it must have been an Indianized form of cedar. The Malecite substituted the word "Stakw-nok", meaning fir, for cedar. Palm Sunday was the day when everyone joined the procession around the inside of the church carrying palms. Weather conditions outside were not conducive to an outdoor procession. Many of the families were still in the bush.

The Malecite remembered Good Friday in two ways. The first, Chib-yawtkw-es-o-kisos, "sun makes sign of the cross": if one looked at the sun, it would swirl around until it made the sign of the cross. In the early 1900's all the youngsters could be seen staring at the sun to see it make the sign of the cross, Peter Paul said.

The other name for Good Friday was "Zam-hoo-dine" or Whipping Time. Early in the morning all the children would get up and go out and cut 7 switches. They took them to their father who would tie them loosely together and whip the youngsters 7 times. During the day everyone would cut his 7 switches and be whipped as punishment for all his unpunished sins. Everyone would fast for breakfast. The whipping and fasting were considered a purification rite.

It is of interest that the Wabanaki used their "magic number 7" in the Good Friday purification-whipping rite. According to tradition, the Wabanaki believed that the seventh son was born with special powers and was considered a doctor. Any Wabanaki could fight a Medoulin by boiling 7 needles. The Wabanaki incorporated the magic of "7" to insure that absolution would be granted to them and all would go to heaven.

Easter, Aw-pi-chi-pek, or "when the leaves begin to grow," might come when there was still plenty of snow on the ground. Usually most families were still in the bush in the winter hunting camps. However, by that time of year the creeks and brooks were usually free of ice and torrents of water were rushing to the rivers, lakes, or ocean. On Easter, before sunrise, all natural running water was holy water and had special healing power if one bathed in it. In 1951 a Woodstock mother of an infant sick with pneumonia took the baby before sunrise and immersed it in an icy brook. That night the baby died. A neighbor woman asked why the mother couldn't have brought the water in and warmed it. Some years later a St. Francis informant indicated that the water was always used cold and never warmed. He added that they no longer followed this custom because most water was known to be polluted. The Malecite believed that the water was holy for the three mornings in the span of Good Friday to Easter.

Corpus Christi, "Bamosseymihamok," was one of the more important Christian festivals for the Wabanaki. It fell about the time all the people had returned to the villages for corn planting. Older people can remember when priests visited the Indians only twice a year. Corpus Christi was one of these occasions. Kingsclear was the only reserve that had a church. The Indians from Tobique, Woodstock, Fredericton and Oromocto all went to Kingsclear to celebrate Corpus Christi. Anyone having thoughts of marriage was encouraged to get married at this time. It was customary for parents to arrange marriages. This was an ideal time for weddings when all the families of the River were assembled.

Altars were set up outside. There would be a grand procession. The priest carried the sacrament. Some people carried banners. The priest stopped at the first altar, put the Sacrament down and said prayers. They proceeded to the next and said more prayers. This was repeated until they arrived at the church. At Old Town, Maine, the procession went to Oak Hill and then returned to the church.

After the service the weddings took place. There were always at least two or three couples to be united. The families of those married provided the marriage feast. Everyone helped with the cooking, the cutting and carrying of firewood, getting water, and all the other chores that made up a good time. Of course, dancing followed the feast and continued until morning. Corpus Christi would be long remembered by the people.

St. John's Day, "Skute Peskasik," was translated as "the time to shoot at the fire". Steve Laurent, a St. Francis informant, insisted that this was the correct translation although no other informants agreed with him. For the Malecite children, St. John's Day, the 24th day of June, was important as the first day that they were allowed to go swimming. It was difficult to find a way to make "Skute Peskasik" fit this significant event. Research has shown that bonfires, the shooting of guns, and shooting into fires were methods of celebrating that Indians accepted soon after the coming of the Europeans.

> "Membertou did gather six or seven barrels of the Corn that he had sowed: and had yet one left which he reserved for the Frenchmen, whom he looked for and whom, arriving, he saluted with three Musket shots and Bonfires." (Levermore 1912: 306).

St. John's Day coincides with the midsummer solstice, a time when people from ancient tradition celebrated with fires in many parts of the world.

> "In San Pedro Manrique, Soria Province, Spain, young men walk across glowing embers on the eve of the midsummer solstice." (Kenny 1972: 384).

At a very early period customs which were a blend of Christian and pagan were introduced on our coast.

> "We may here consider a bad custom of leaping over the fire, and of making children pass through the flames of

> the bonfires upon St. John the Baptist's day; which custom endures to this day among us, and ought to be reformed ..." (Lescarbot 1907: Vol. III-109).

The Wabanaki name for St. John's Day is evidence that the French introduced them to the custom of bonfires and shooting into the fire. The Jesuits attempted to curtail this pagan practice and would not participate in it. (Thwaites Vol. 35-35, 51). Since the Wabanaki were served by Jesuits after 1646, this custom was abolished but the name persists. In Quebec Province St. John's Day is still widely celebrated with bonfires, and in some places such as Mount Royal, Montreal, with fire works as well.

St. Anne's Day or "See-dan-o-win," is perhaps the most important Wabanaki Christian holiday. Since 1629 St. Anne has been the Patron Saint of the Wabanaki. The Jesuits Vimont and Vieuxpoint had promised their patroness, Anne of Austria, Queen Mother of France, that they would dedicate their first chapel in the New World to Sainte Anne d'Apt, which they did. This Saint Anne should not be confused with Saint Anne de Beaupre. (Wallis 1955: 183). Many Wabanaki Indians claim that their Patron Saint has assisted new born infants, given care to ailing mothers, and provided help for others with medical problems. The annual celebration dates from 1742, when the Rev. Abbé Maillard first preached to the Micmac people at Chapel Island. He used a rock as a pulpit which is still a tourist attraction. The Church boasts a statue of St. Anne that was found hidden in the woods during the seige of Louisbourg in 1745, and an altar dated 1717, also hidden in the woods during the Battle of Louisbourg. The altar was put together with interlocking pieces, no nails being used in its construction. Currently the festivities encompass several days. (Pacifique 1935: 52). Now the Malecite and Passamaquoddy celebration of St. Anne's Day takes place at Tobique, but it is not thought to have been celebrated there before 1904. Laurent does not include St. Anne's Day in his list of Holidays and Festivals (Laurent 1884: 56). So it may be a recent introduction at St. Francis. Although the Mass and Procession of St. Anne are the central events, sports competitions and Indian dances are popular. St. Anne's Day is also noted for weddings.

Christian holidays and festivals have been a way of Wabanaki life for over 350 years.

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# A PRELIMINARY REPORT ON THE DIALECTS OF EASTERN OJIBWA-ODAWA

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The dialect situation in Southern Ontario and the lower peninsula of Michigan has never been particularly clear with respect to a group of mutually intelligible dialects known variously as Chippewa, Ojibwa, Odawa, and, to linguists, Eastern Ojibwa. Several years work in this region with several speakers makes possible some hypotheses and some tentative conclusions regarding the distribution of dialects and major dialect features. The work leading up to this report was done for reasons other than dialect study, therefore the results should be considered somewhat tentative. However, it is hoped that this report will provide a starting point for any who would be interested in further investigation.

The dialects in question are characterized by a deletion of short, unstressed vowels. They are spoken in the lower peninsula of Michigan and in Ontario on the north shore of Georgian Bay, on Manitoulin Island, and south and east of Lake Huron and Georgian Bay. Among older speakers in the northern most areas (including Manitoulin Island) the deletion seems to be a kind of casual speech phenomenon, and the vowels can be easily resupplied (with only some ambiguity of quality). For younger speakers everywhere, and everyone in southern areas, the vowels deleted are totally abstract. They cannot resupply the vowels except by reference to appropriate allomorphs even though the use of open transitions in some places from which vowels have been deleted is common.[1]

(1) (a) /gi:namadabi/ He sat./He was sitting

Western dialects [gí:namádabí]

Older eastern speakers [gi:nmʌdᵊbɛ] (normal)
[gí:nəmʌ́dəbɛ́] (careful)

Southern speakers [gi:nmʌdbɛ] or
[gi:nᵊmʌdᵊbɛ]

(b) /baba:mose:/ He's walking around./He's going for a walk.

| | | |
|---|---|---|
| Western dialects | [babá:mosé:] | |
| Older eastern speakers | [pa:mse:] | (normal)[2] |
| | [bᵊbá:mᵘsé:] | (careful) |
| Southern speakers | [pa:mse:] | (only way) |

Within the dialect area just outlined, there are essentially two dialects.[3] The central dialect is spoken west of a line running north-south through the Bruce peninsula,[4] and including all of Manitoulin Island. The eastern dialect is spoken east of that line.

The differences between these two dialects while for the most part being relatively minor cover the total spectrum of the parts of the language: phonological, morphological, syntactic, and lexical. The next section of this paper will be devoted to exemplifying some of these differences. While it may turn out that not all of the features listed below will uniquely characterize one dialect over against the other,[5] it is hoped that they will be useful to others working in this area.

The most striking difference between the two dialects is in the determiner system and the question word system. Therefore these systems will be treated first and separately from the rest of the data.

(a) determiner systems[6]

(a) central dialect

| | animate | | inanimate | | locative |
|---|---|---|---|---|---|
| | sing. | plu. | sing. | plu. | |
| nearer | ma:ba | gonda | ma:nda | ninda | ma:mpi: |
| further | aw | giw | iw | niw | ma: |

| | animate | | inanimate | | locative |
|---|---|---|---|---|---|
| | sing. | plu. | sing. | plu. | |
| remote | we:di | giwi | we:di | niwi | wadi |

(b) eastern dialect

| | | | | | |
|---|---|---|---|---|---|
| nearer | ahaw | gow | ow | now | o:ma: |
| further | aw | giw | iw | niw | ma: |
| remote | we:di | giwe:di | we:di | niwe:di | widi |

Aside from the fact that many speakers are losing the three way distinction here represented, there are speakers who know all of one system and parts of the other. Bloomfield's informant (Bloomfield (1957)) is an example. He basically used the central system but knew the remote forms of the eastern dialect, as well as ow (= ma:nda). At the end of this paper we will return to this fact and explain it. Before leaving this discussion two further forms should be added, živi (= wadi) and žanda (= ma:mpi:), which are characteristically Odawa, although all central speakers seem to know them, whether or not they use them.

(3) question words[7]

| | central | eastern |
|---|---|---|
| what (anim.) | we:ne:n | we:ne:n |
| what (inan.) | we:ne:n | we:gne:n |
| how | a:ni:n/a:ni: | a:ni:n |
| when | a:ni:š pi:/<br>a:ni:š pič | a:ni: pi:[8] |
| where | a:ni: pi:š/<br>a:pi:š | a:ndi |
| why | a:ni:n daš[9] | we:gne:n daš[9] |

There is one syntactic process which affects question words and which deserves mention here. The word *daš* 'then, (and) so' often appears in content questions. It is normally contracted with the preceding word which is, in the case of questions, the question word. The possible contractions are given in (4).

(4)

| plain form | contracted form |
|---|---|
| we:ne:n | we:ne:š |
| we:gne:n | we:gne:š |
| a:ni:n | a:ni:š |
| a:ndi | a:ndiš |

It is interesting to note that the central words for *when* and *where* involve lexicalized contractions. The expressions for *why* may not contract unless they are in a full sentence to avoid synonymy with *how* in the central dialect and *what* in the eastern dialect.

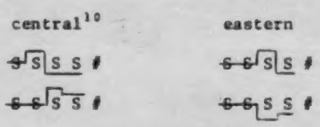
*Phonological Differences*. There are phonological differences both of the low-level type and of the more abstract type. We start by examining some low-level differences.

(5) Vowel Placement (long vowels)

| | central | eastern |
|---|---|---|
| /i:/ | [i:] | [i:] |
| /e:/ | [ɛ:] ~ [e:] | [æ:] |
| /o:/ | [o:] ~ [u:] | [o:] |
| /a:/ | [a:] ~ [ɔ:] | [ɔ:] |

In addition to the difference in vowel placement, apicals and spirants are often treated differently. Eastern speakers tend to have alveolar apical stops, while central speakers tend to have dental apical stops. Central speakers tend to use retroflex [ṣ̌], while eastern speakers use [š].

(6) Intonation (S = syllable, # = end of sentence)



|  | central[10] | eastern |
|---|---|---|
| Neutral | ~~S~~ S S S # | ~~S S~~ S S # |
| Question | ~~S S~~ S S # | ~~S S~~ S S # |

Of the low-level differences there remain a number of miscellaneous systematic differences in pronunciation that seem to be captured best as rules. There also does not seem to be any better way to present them than to simply list them. One additional note seems necessary, at this level, lenis and fortis consonants neutralize syllable finally in a way that is capturable most naturally by treating fortes as voiceless and lenes as voiced.

Central speakers neutralize the lenis fortis contrast word finally.

(7) $[+\text{obs}] \rightarrow [-\text{vd}]\ /\ \_\_\#$

Some examples follow.

(8)

|  | central | eastern |  |
|---|---|---|---|
| b/p | ži:ši:p | ži:ši:b | duck |
|  | mba:p | n̩ba:p | I laugh |
| d/t | ŋgi:git | n̩gi:gid | I speak |
|  | nit | nit | spear |

| | central | eastern | |
|---|---|---|---|
| g/k | ptik | m̩tig | tree |
| | mik | mik | beaver |
| z/s | mbo:s | ŋ̩bo:z | I embark |
| | mba:s | ŋ̩ba:s | I am dry |

Two comments can be made about these facts. First, consonants in final position are released in both dialects. One effect of this is that the voiced consonants (i.e. lenes) of the eastern dialect often have a voiceless release, and in fact sound like a cluster of voiced plus voiceless aspirated, e.g. [$bp^h$] for /b/. Secondly, the z of both dialects is often weakly voiced, especially at the beginning and end of words. Nonetheless there is a voicing contrast in the eastern dialect, at the end of words.

Central speakers lower word final short i.

(9) $i \rightarrow \varepsilon$ / ___#

Some examples follow.

(10)

| central | eastern | |
|---|---|---|
| ninε | nini | man |
| pičε | piči | robin |
| ni:mε | ni:mi | he dances |
| a:wε | a:wi | he is (something) |

This feature was noted by Bloomfield, but it is characteristic only of central speakers.

Central speakers back short a in the sequence wag, or wak, or wan

(11) a → ɤ / w __ [+velar]

| (12) | central | eastern | |
|---|---|---|---|
| | ni$^{\cup}$wɤk | nin$^{\cup}$wag | men |
| | da:ŋkškawɤk | da:ŋgškawag | (that) I touch him |
| | be:ž$^{\cup}$wɤk | be:ž$^{\cup}$wak | (that) it is near |
| | gmiwɤŋ | gmiwaŋ | (that) it rains |

Actually, it is probably the eastern dialect that is out of line here. Baraga often writes wog for wag which probably reflects the effect of (11) in his western dialect.

Central speakers devoice obstruents in front of voiceless consonants.

(13) [+obs] → [-vd] / ___ [-vd]

This rule is used more by southern speakers than by northern speakers. It is also beginning to operate optionally in the speech of some eastern speakers.

| (14) | central | eastern | |
|---|---|---|---|
| b/p | bmipto: | bmibto: | he runs |
| d/t | wa:tkohge: | wa:dkohge: | he waves |
| ǰ/č | mbana:čto:n | n̩bina:ǰto:n | I break it |
| g/k | bi:kška:[11] | bi:gška: | it is torn |
| z/s | ã:skon$^{\llcorner}$ye: | ã:zkon$^{\llcorner}$ye: | he changes clothes |
| z/s | do:što:n | w̩do:žto:n | he makes it |

The effect of rule (13) is to reduce the number of contrastive clusters. Theoretically there are five, e.g. for ž/š plus g/k: žg (underlying), žg (derived, by vowel deletion), žk (derived, by vowel deletion), šk (derived by vowel deletion), and šg (derived by vowel deletion). In fact there are only three on the surface, by (13) žk → šk, and the underlying lenis cluster žg also merges with derived šk because it is not voiced. Thus the three surface clusters are: šk (three sources), žg, and šg.[12]

Younger central speakers optionally delete w between vowels.

(15) w → $\emptyset$ / V ___ V (optional)

This is a casual speech phenomenon. When speakers who use (15) are asked to slow down, or to speak carefully, the w's reappear.

| (16) central casual | central careful | eastern | |
|---|---|---|---|
| wa:a:bgono:ji: | wa:wa:bgono:ji: | wa:wa:bgono:ji: | mouse |
| nda:i:sin | nda:wi:sin | nda:wi:sin | I should eat |
| gi:ɛ:ɤk | gi:wɛ:wɤk | gi:wa:wag | they go home |

Notice that the last example in (16) shows that (15) is ordered after (11) which backs a after w.

Eastern speakers nasalize vowels in the sequence vowel - semivowel - nasal - consonant. Vowel length is irrelevant and the sequence of consonants may arise as the result of vowel deletion.

(17) $V \rightarrow [+\text{nas}] \;/\; \_\_ \begin{Bmatrix} y \\ w \end{Bmatrix} \begin{matrix} C \\ [+\text{nas}] \end{matrix} \; C$

(18)

| | | central | eastern | |
|---|---|---|---|---|
| underlying cluster | | gi:nǰiba: | gĩ:w̃ǰiba: | he came from (there) |
| | cf. | nǰiba: | wnǰiba: | he comes from (there) |
| derived cluster | | wi:snimba | wi:snĩỹban | (that) you ate |
| | cf. | bo:zyamba | bo:zyamban | (that) you embarked |

(endings = yan 'you(sg.)' + bani 'preterite')

Some eastern speakers have a rule that moves nasalization leftward onto a long vowel from a deleting nasalized short vowel.

(19) $$V\!: \rightarrow [+\text{nas}] \;/\; \_\_\ C \begin{bmatrix} V \\ -\text{long} \\ +\text{nas} \end{bmatrix}$$

The rule can be formulated in this form because the presence of the long vowel conditions the deletion of the following short vowel in all cases except where that vowel is word final. But there are no word final short nasalized vowels.

(20)

| | central | eastern | |
|---|---|---|---|
| | be:žgo:gži: | be:žgõ:gži: | horse |
| | /be:žigo:-ganži-e:/ | | (he has one hoof) |
| | ŋgike:ndzi:n | n̩gikẽ:ndzi:n | I (don't) know |
| | /ni-gik-e:nd-am-si:-n/ | | |
| cf. | ŋgike:nda:n | n̩gike:nda:n | I know |

Not only is this rule unusual, it is of some theoretical interest because it amounts to a 'look-ahead' rule. However, it is likely that it is limited to an area smaller than the whole eastern area. It was first noticed in a speaker from Curved Lake.

Now let us consider some of the more abstract phonological rules differentiating the two dialects.

Perhaps the single best diagnostic for differentiating central from eastern speakers is the rule that central speakers have which deletes n from an ad hoc list of inflectional suffixes and pronouns.

(21) n → ∅ / ___ # irregularly

This rule operates consistently in suffixes. But distinguishes unstressed from stressed pronouns.

| (22) | | central | eastern | |
|---|---|---|---|---|
| suffixes | | nwa:bma:ba | nwa:bma:ban | I saw him |
| | cf. | nwa:bma:bni:g | nwa:bma:bni:g | I saw them |
| | | nwa:bma:na: | nwa:bma:na:n | we see him (exc.) |
| | cf. | nwa:bma:na:nig | nwa:bma:na:nig | we see them (exc.) |
| | | ngi:we:mi | ngi:we:min | we go home (exc.) |
| | cf. | ngi:we:mna:ba | ngi:we:mna:ban | we went home (exc.) |
| pronouns | | ni: | ni:n | I |
| | | ni:nwi | ni:nwin | we (exc.) |
| | | gi: | gi:n | you (sg.) |
| | | gi:nwi | gi:nwin | we |
| | | wi: | wi:n | he |
| stressed | | ni:n | -- | I |
| pronouns | | gi:n | -- | you (sg.) |
| | | wi:n | -- | he |

Central speakers delete w from preconsonantal position which comes about as the result of vowel deletion.

(23) w → ∅ / ___ C

(24)

| | | central | eastern | |
|---|---|---|---|---|
| (a) | | do:da:ba:nan | wdo:da:ba:nan | his car |
| | | minpida:n | wminpida:n | it tastes good to him<br>(lit. he tastes it good) |
| (b) | | wi:snidig | wi:sniwdog | he supposedly eats |
| | | wi:sniba | wi:sniwban | he ate |
| | cf. | bo:zwidig | bo:zwidog | he supposedly embarks |
| | | bo:zwiba | bo:zwiban | he embarked |
| (c) | | ŋgi:žido:n | ŋ̩gi:žiwdo:n | I took it (there) |
| | cf. | ndižwido:n | n̩dižwido:n | I take it (there) |

Rule (23) operates uniformly only in casual speech. The hierarchy of environments is given in (24). For all central speakers (23) operates word initially in all speech styles, e.g. (24a). For most central speakers (23) operates in all speech styles when the following consonant belongs to one of the two modal suffixes, bani preterite and dig dubitative, e.g. (24b). For younger central speakers in casual speech, (23) operates everywhere, e.g. (24c).

Central speakers nasalize long vowels while deleting n by (21).

(25) V: → [+nas] / ___ n # irregular

| (26) | | central | eastern | |
|---|---|---|---|---|
| /-a:n/ | | wi:sniyã: | wi:sniya:n | (that) I eat |
| (that) I | | wa:bminã: | wa:bmina:n | (that) I see you |
| | cf. | nwa:bma:na:[13] | nwa:bma:na:n | we see him (exc.) |
| | cf. | wi:sniya:mba: | wi:sniya:mba:n | (that) I ate |
| /-e:n/ | | wi:sniya:nẽ: | wi:sniya:ne:n | (that) I supposedly eat |
| dubitative | | wa:bma:wdẽ: | wa:bma:wde:n | (that) you supposedly see him |
| | cf. | wi:snidige:nag | wi:sniwdoge:nag | they supposedly eat |

In addition to the two endings, a:n conjunct first person singular and e:n dubitative, the prohibitative ke: conditions both (21) and (25).

| (27) | | central | eastern | |
|---|---|---|---|---|
| | | ni:mkẽ: | ni:mke:n | (do not) dance |
| | cf. | ni:mke:gon | ni:mke:gon | (do not) dance (pl.) |

Apparently the appearance of (25) is recent and (25) is spreading eastward. Bloomfield (1957) contains a text discussing the effect of (25), in which the informant says that (25) was used in Saginaw but not on Walpole. The text was given in 1938. Nowadays speakers on Walpole use (25) consistently.

Central speakers denasalize syllabic n's.

(28) n → ɨ

The source of syllabic nasals is the sequence consonant-short vowel-n-consonant. When the short vowel is deleted the n syllabifies and, in the central dialect, that n denasalizes.[14]

| (29) | central | eastern | |
|---|---|---|---|
| | dɨgiškwa: | dŋgiškwa: | he gets kicked |
| | ne:ndɨgẽ: | wne:ndŋ̩ge:n | (don't) forget |
| | dbe:nɨdizo | dbe:n̩dizo | he's on his own |
| | mbazɨda:n | n̩bizn̩da:n | I listen to it |

Kaye (1973) discusses this rule (28) briefly.

Eastern speakers delete the w off the end of determiners when they precede the nouns they modify.

(30) w → ∅ / ___ # N  (in determiners)

| (31) | central | eastern | |
|---|---|---|---|
| | aw nini | a nini | that man |
| | (ma:nda ki) | o ki | this ground, the earth |
| | iw mo:kma:n | i mo:kma:n | that knife |

Eastern speakers treat the morpheme minw 'good, well' irregularly, deleting the w before vowel initial morphemes and morphemes that begin with deletable w (i.e. we:n 'use the mind' and wa:b 'see, light').

(32) w → ∅ / ___ V  (in the morpheme minw)

| (33) | central | eastern | |
|---|---|---|---|
| | nmin,wa:bmina:goz | nmina:bmina:goz | I'm good looking |

| central | eastern | |
|---|---|---|
| mnywa:gmi | mna:gmi | it tastes good (liquid) |
| nminob | nminab | I'm comfortable (sitting) |

Central speakers have a rule (or complex of rules) that irregularly contract wa and wi to o. The contraction of wi to o is more irregular than the change of wa to o. Because there is an irregular change of i to a in the central dialect (which we will discuss below) we will treat the change of wi to o as a two step process: wi → wa → o. For the most part correspondences between the two dialects represent restructuring in the central dialect.

(34) $wa \rightarrow o \;/\; C \_\_ \begin{Bmatrix} C \\ \# \end{Bmatrix}$

(35)

| central | eastern | |
|---|---|---|
| žomin | žwimin | grape |
| wa:ga:kot | wa:ga:kwad | axe |
| mko | mkwa | bear |
| mnopgot | mnopgwad | it tastes good |

The contraction of wa and wi to o is a very difficult analytical problem. There was a rule of Proto Algonkian.

(36) $^*we \rightarrow {}^*o$

the reflex of which in all dialects of Ojibwa is

(37) $wi \rightarrow o$

because of the change from PA to Ojibwa *e > i. Consider the evidence for contraction (37) from the eastern dialect given in (38).

| (38) | II | AI | |
|---|---|---|---|
| | wa:bškad | wa:bškizi | it is white |
| | a:bdad | a:bdizi | it is useful |
| | mnopgwad | mnopgozi | it tastes good |
| | mskwa: | mskozi | it is red |

Therefore, in view of this problem, we will pass over a more detailed analysis leaving that for future investigation.

The final abstract rule that will be considered here involves a complex restructuring in the eastern dialect. The rule in question deletes *y* before *i* and *w* before *o*. The central dialect also has a version of this rule but relative order of the rules is different.

(39) $\begin{Bmatrix} y \\ w \end{Bmatrix} \rightarrow \emptyset \ \_\_ \ \begin{Bmatrix} i \\ o \end{Bmatrix}$

Consider the following evidence for the *w* part of the rule in the central dialect.

| (40) | father | friend |
|---|---|---|
| my | no:s | ni:ka:nis |
| your | go:s[15] | gi:ka:nis |
| his | o:san | wi:ka:nsan |

There is similar direct evidence in the eastern dialect for the *w* part of (39).

| (41) | grandmother |
|---|---|
| my | no:kmis |
| his | o:kmisan[16] |

However the complexity in the eastern dialect arises in the interaction of this rule with the vowel deletion rule, and with a rule that deletes *h* preceding a semivowel. The operation of the *h* deletion rule is best seen in the allomorphs of *ahw* a TA final meaning 'act with an instrument'. When the *w* contracts with a following *i* by (37) the *h* remains as in (42a). When the *w* remains as *w* the *h* is deleted as in (42b).

(42) (a) ŋgi:bo:knike:hok — he broke my arm
/ni-gi:-bo:kw-nike:-ahw-igw/

(b) ŋgi:bo:knike:wa: — I broke his arm
/ni-gi:-bo:kw-nike:-ahw-a:/

The *h* deletion rule is given in (43).

(43) $h \quad \emptyset \ / \ \_\_\_ \begin{Bmatrix} y \\ w \end{Bmatrix}$

Now in the eastern dialect the deletion of *y* and *w* follows the vowel deletions. (In addition the contraction of (37) works a little differently as we shall see.) The following data illustrate this.

| (44) | central | eastern | |
|---|---|---|---|
| | ŋgi:bo:knike:hok | ŋgi:bo:knike:hog | he broke my arm |
| | ŋgi:bo:knike:hgo:k | ŋgi:bo:knike:wgo:g | they broke my arm |

To make it clear how this works let us derive the eastern form ngi:bo:knike:wgo:g they broke my arm next to the form ngi:bo:knike:hog he broke my arm.

| | | |
|---|---|---|
| (45) | Underlying | ni-gi:-bo:kw-nike:-ahw-igw-ag |
| | Other rules | nigi:bo:kwinike:hwigo:g |
| | Revised (37)[17] | nigi:bo:kwonike:hwogo:g |
| | Vowel Deletion | ngi:bo:kwnike:hwgo:g |
| | h Deletion (43) | ngi:bo:kwnike:wgo:g |
| | Other rules[18] | ngi:bo:knike:wgo:g |

| | | |
|---|---|---|
| (46) | Underlying | ni-gi:-bo:kw-nike:-ahw-igw |
| | Other rules | nibo:kwinike:hwig |
| | Revised (37) | nibo:kwonike:hwog |
| | Vowel Deletion | nbo:kwnike:hwog |
| | (39) | nbo:kwnike:hog |
| | Other rules | nbo:knike:hog |

Now consider the following forms which show that eastern speakers have restructured instances of hi as hyi and instances of ho as hwo.

| (47) (a) hi hyi | | central | eastern | |
|---|---|---|---|---|
| | (i) | nmawha: | nmo:ha: | I make him cry |
| | | nmawhigo:k | nmo:ygo:g | they make me cry |
| | | mawih | mo:y | make him cry[19] |
| | (ii) | ŋgi:we:ha: | ngi:we:ha: | I send him home |
| | | ŋgi:we:hgo:k | ngi:we:ygo:g | they send me home |
| | | gi:we:h | gi:we:y | send him home |

| hi hyi | central | eastern | |
|---|---|---|---|
| (iii) | ŋgi:mniha: | n̩gi:mnaha: | I gave him a drink |
| | ŋgi:mnihgo:k | n̩gi:mnaygo:g | they gave me a drink |
| | mnih | mnay | gave him a drink |
| (b) ho hwo | | | |
| (i) | nminmanǰhomi | nminmanǰhomin | we (exc.) feel good |
| | nminmanǰah | nminmanǰiw | I feel good |
| | ŋgi:mnomɨǰahmi | n̩gi:mnam̩ǰiwmin | we (exc.) felt good |
| (ii) | mba:šǩǰi:hok | n̩ba:šǩǰi:hog | he crushes me |
| | mba:šǩǰi:hgo:k | n̩ba:šǩǰi:wgo:g | they crush me |

The final phonological facts that we will mention concern two characteristic sounds shifts of the central dialect.

First i shifts irregularly to a.

| (48) | central | eastern | |
|---|---|---|---|
| | ndakwe:m | n̩dikwe:m | my woman, my wife |
| | mbazɨda:n | n̩bizn̩da:n | I listen to it |
| | nminmanǰah | nminmanǰıw | I feel good |

Some instances of this are listed in Baraga and characterized as Odawa. The vowels that are most susceptable to this shift are those that are deleted in the most statistically common allomorphs and those that appear post-tonically, i.e. in the penultimate and final syllables.

Secondly o shifts to a in casual speech, except after velars.

| (49) | | central<br>casual | central<br>careful | |
|---|---|---|---|---|
| | (a) | mase: | bmose: | he walks |
| | | žamna:bo: | žomna:bo: | wine |
| | | mba | mbo | he is dead |
| | (b) | pko | mko | bear |
| | | mnapgozi | mnopgozi | it (anim.) tastes good |

*Morphological Differences*. There are four morphological differences which have been noticed. We simply list them.

Central speakers contract the sequence of *ni* I and *wi:* intentive future to *ni:*.

| (50) | central | eastern | |
|---|---|---|---|
| | ni:ža: | nwi:ža: | I will go |
| | gwi:ža: | gwi:ža: | you will go |
| | wi:ža: | wi:ža: | he will go |
| | gwi:ža:m | gwi:ža:min | we will go (inc.) |
| | ni:ža:m | nwi:ža:min | we will go (exc.) |

In transitive inanimate verbs central speakers use *na:ni* to mark first person plural, eastern speakers use *min*.[28]

| (51) | central | eastern | |
|---|---|---|---|
| | mbo:kbido:na: | nbo:kbido:min | we break it (exc.) |
| | gbo:kbido:na: | gbo:kbido:min | we break it (inc.) |
| | gbo:kbido:na:nin | gbo:kbido:min | we break them (inc.) |

Central speakers "borrow" the form that means 'you are acted upon' and use it for 'we act on you'. The borrowed forms do not neutralize the plurality of the second person even though the original forms did.

| (52) |  | central | eastern |  |
|---|---|---|---|---|
|  | (a) | gwa:bam | gwa:bam | you see me |
|  |  | gwa:bmim | gwa:bmim | you (pl.) see me |
|  |  | gwa:bmimi | gwa:bmimin | you see us |
|  |  | gwa:bmimi | gwa:bmimin | you (pl.) see us |
|  | (b) | gwa:bmin | gwa:bmin | I see you |
|  |  | gwa:bminim | gwa:bminim | I see you (pl.) |
|  |  | gwa:bmigo: | gwa:bminmin | we see you |
|  |  | gwa:bmigo:m | gwa:bminmin | we see you (pl.) |
|  | (c) | nwa:bmigo: | nwa:bmigo: | I am seen |
|  |  | gwa:bmigo: | gwa:bmigo: | you are seen |
|  |  | gwa:bmigo:m | gwa:bmigo:m | you (pl.) are seen |

Some morphemes have slightly different spellings not due to sound change. The list of examples is not intended to be exhaustive.

| (53) | central | eastern |  |
|---|---|---|---|
|  | mi:nzis | wi:nzis | (a) hair |
|  | ba:gha:kwa | ba:gha:kwe: | chicken |
|  | i:dig | i:dog | dubitative markers |
|  | -dig | -dog |  |

Syntactic Differences. The syntactic differences noticed are fewer and have not been extensively corroborated.

Some eastern speakers omit obviation mark(s) on fronted nouns.

(54) (a) m̩bil wgi:wa:bma:n n̩ǰa:n — John saw *Bill*.

(b) wgi:wa:bma:n m̩bili:n n̩ǰa:n — John saw Bill.

(mbili:n = mbil + an obviative)

Some eastern speakers omit the *n* of the pseudo-transitives derived from intransitive verbs.

(55) (a) nminkwe: — I'm drinking.

(b) nminkwe: i žwimna:bo: — I'm drinking the wine.

Some eastern speakers omit second plural marker in transitive animate verbs when the referent is unemphasized.

(56) (a) n̩gi:wa:bma:na:n — We (exc.) saw him/them.

(b) gi ninwag n̩gi:wa:bma:na:nig — We (exc.) saw *those* men.

*Lexical Differences*. There are a relatively large number of lexical differences between the two dialects. We will simply list some examples including differences in conversational items (57a) and other differences.

| (57) |  | central | eastern |  |
|---|---|---|---|---|
|  | (a) | ma:ǰa:n | (wn)da:s ma: | Come here. |
|  |  |  | cf. ma:ǰa:n | Go away, get lost! |
|  |  | wa: | way | huh? |

| | central | eastern | |
|---|---|---|---|
| | a: | ambe: | Let's go, come on. |
| | ma:nda | m̩be: | Here, here it is. |
| (b) | no:s | n̩de:de:m | my father |
| | mbaba: | -- | my dad |
| | ŋgaši | n̩do:do:m | my mother |
| | nmama: | -- | my mom |
| | mi:kno:t | bodye:gwa:zon | trousers |
| (c) | gči-ža:ži | me:wža | a long time ago |
| | a:bdik | po:ǰ | necessarily |

cf. central : a:bdik ˈŋgawanoki:
eastern : po:ǰ n̩gawinoki: I have to go to work

n̩ganoki: I will work

n̩gawinoki: I will go to work

| | | | |
|---|---|---|---|
| (d) | bi:da:zbi:t | be:gde:d | to belch[21] |
| | bi:nda:kwe:t | zgaswe:d | to smoke |
| | ga:čiŋ | ga:šī:waŋ | to be small (II) |
| | ga:čī:hit | ga:šī:wid | to be small (AI) |
| | gkide:k | ga:de:g | to be hidden (II) |
| | gkizot | ga:zod | to be hidden (AI) |
| | gkido:t | ga:do:d | to hide s.t. (TI) |
| | gkina:t | ga:na:d | to hide s.o. (TA) |
| | mbwa:čha:t | mwadsa:d | to visit s.o. (TA) |
| | za:gǰi:taŋ | za:ghaŋ | to go out(side) |

Conclusion. At this point the evidence is clear that there are two dialects in the vowel deleting area. Even a cursory examination of Bloomfield (1957) will show that his speaker was basically a central speaker. He used the central determiner system, most of the central question words, showed many of the central phonological rules, used many of the central morphemes and lexical items. However, he did not use all of them, and this appears to be a problem. In fact this apparent problem turns out to be an interesting fact of linguistic history. Consider the following data from Baraga (1878) (in edited spelling) which suggest strongly that the central dialect is the descendent of the dialect spoken by the Odawa people.

(58)

| modern | Baraga | modern | Baraga | |
|---|---|---|---|---|
| central | Odawa | eastern | Ojibwa | |
| ndip | ondib | wštigwa:n | oštigwa:n | his head |
| ni:bna | ni:bina | ni:bwa | ni:biwa | much |
| mno:kmi | mino:kami | zi:gwan | zi:gwan | it is spring |
| gkizo | gakizo | ga:zo | ga:zo | he is hidden |
| sina:miž | asina:miž | nina:tig | inina:tig | maple tree |
| sipnigan | asiponigan | mo:žwa:gan | mo:žwa:gan | scissors |
| ža:ži | ža:žaye: | me:mža | me:wiža | long ago |
| ŋgaši | ningaši | ndo:do:m | ningi: | my mother |
| ndakwe:m | nindakwe:m | ndikwe:m | nindikwe:m | my woman |

However, many speakers of the central dialect are from Ojibwa families. Many of the older speakers of the central dialect know eastern (i.e. Ojibwa) forms even though they don't normally use them. In fact many of the oldest speakers in this group have "Ojibwaisms" in their speech. Almost certainly Bloomfield's speaker is among these. He came from an Ojibwa family but grew up in an Odawa area. Apparently Ojibwa people moved into Odawa areas and "shifted" dialects.

In view of this one would expect to find less consistency in the central dialect than there appears to be. This also explains why speakers on Manitoulin say that they speak a mixture of Odawa and Ojibwa, when linguistically they speak Odawa.

The final issue that needs to be mentioned regards the naming of the two dialects. Because of the way the dialects run, Bloomfield's term 'Eastern Ojibwa' is inappropriate to the dialect it was coined to name. Because of the feelings of the Odawa and Ojibwa people about how their language should be named, I feel it is most appropriate to call the central dialect Central Ojibwa-Odawa, and to reserve the term Eastern Ojibwa for the eastern dialect.

## NOTES

[1] Except in the places where phonetic detail is relevant, forms will be cited in a "phonemic" notation of the sort that should be obvious to any Algonkianist. In particular this means that the abstract vowels will be omitted. In effect the transcription amounts to a loose phonetic rendering.

[2] Clusters of identical lenis obstruents word initially contract to a fortis consonant. Bloomfield claims that there is a contrast between an initial fortis consonant and a cluster of identical lenes at the same point of articulation (1957, § 1.11 pg. 8), but that contrast has been lost in the speech of many speakers.

[3] This statement has to be hedged somewhat because there seem to be many localisms. For example, in the dialect including Manitoulin Island the word meaning 'to smoke' is bi:nda:kwe:d, whereas the other dialect uses zgaswe:d. But on Manitoulin bi:na:kwe:d is now considered old fashioned and zgaswe:d is the word normally used.

[4] Unfortunately, data from Cape Croker is lacking at this time. Therefore it is impossible to be more specific. The folk analysis suggests that it is in the eastern area. Eastern speakers say that people in Cape Croker speak the same as they do, while speakers in the central area say that people in Cape Croker speak differently.

[5] Again there are some localisms. On Manitoulin Island the forms wa and wi are used for aw and iw respectively.

[6] Localisms are ignored in this table.

[7] In addition to differences in question words, there are differences in the use of initial change in questions in the two dialects. These are summarized in the following table.

| (59) | central | eastern |
|---|---|---|
| who? | change | change |
| what? | change | change |
| how? | change | change |
| when? | change | plain |
| where (at)? | change/plain | plain |
| where (to)? | change | change |
| why? | change | change |

[8] The emphatic daš cannot be used with this phrase. See the discussion below.

[9] This is the form that appears in elliptical questions, "Why?". In the non-elliptical form the questions look as follows.

(60) central : A:ni:n ga:-nǰi-gi:we:yan? Why did you go home?
eastern : We:gne:n ga:-wnǰi-gi:we:yan?

The preverb (w)nǰi means 'for a reason', so the literal translation of the central question is: Which is the reason..?; the literal translation of the eastern question is: What is the reason..?.

[10] The situation is more complicated in the central dialect than this suggests. Kaye (1973) discusses the phenomenon at length. However, he calls it stress rather than the pitch or pitch accent which it is.

[11] Notice that the voiceless cluster of lenes devoices the preceding lenis.

[12] It is the facts relating to these clusters which show that voicing rather than lenis-fortis which is the crucial fact about the consonants. If we were to treat voicing as derived, the rule voicing lenes would have to precede the vowel deletion to get these clusters to come out right. But even then a devoicing rule would be needed to get the form bi:kǩka: in (14).

[13] This form shows a slight allophonic nasalization of the final vowel, but there is still a clear contrast with the nasalized vowel in wa:bminã:.

[14] The argument supporting this analysis is too intricate to go into here, but it depends on the fact that when the syllabic n assimilates to a preceding labial it remains as a syllabic m and does not denasalize, e.g., nwa:bmda:n.

[15] The an is the mark of the obviative which is obligatory in animates with a third person possessor.

[16] Some eastern speakers have restructured this form as wno:kmisan.

[17] The revision is

(61) i → o / w___

[18] Lest the reader think that the rule needed to delete the w in the environment C ___ C is an ad hoc rule forced by the revision of (37) as (61), consider the forms

| | | |
|---|---|---|
| (62) | bi:nna:gwad | it looks clean |
| | bi:nna:gdo:n | they (inan.) look clean |

[19] The suffix meaning 'act on him' (imperative) is i which is deleted by a regular rule.

[20] Rogers (1975) lists both forms for Parry Island Ojibwa.

[21] Verbs are cited in 3rd singular conjunct forms as is the custom of conservative speakers.

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# HISTORIC POPULATIONS OF NORTHWESTERN ONTARIO

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This paper was prompted by the continuing emphasis placed by some ethnologists on the rather vague historical references to Assiniboine peoples in northwestern Ontario as evidence of their occupation of portions of the area. The prime purpose of the paper is to examine the historical record. It is recognized that the early archaeological literature dealing with Minnesota and Manitoba (Wilford 1941, 1945, 1955; MacNeish 1954, 1958) strongly influenced the approach in equating the Terminal Woodland period Blackduck tradition as an exclusive product of the Assiniboine. This was prior to the record of the presence of the tradition in Ontario from the Severn River in the north, south to the border region of west of Lake Superior and east to roughly the Ottawa river region (Conway 1975; Dawson 1974, 1975a, 1975d; Hurley and Kenyon 1972; Kenyon 1961; Pollock and Noble 1975; Ridley 1954,1966; Wright 1965, 1966, 1968c, 1972, n.d.). The extensive spacial and temporal depth of the tradition and its presence into historic times in this area historically dominated by Algonkian speaking peoples negates Assiniboine affinities in Ontario. Further it is implicit in the record that the Algonkian speakers are not recent arrivals to northwestern Ontario. Information pertaining to the early historic period is reviewed in this paper and the results of the extensive archaeological records are introduced concluding that northwestern Ontario has been occupied from time out of mind by Algonkian speaking peoples.

In attempting to identify the indigenous populations resident in northwestern Ontario at the time of European contact, much of the earliest information respecting the area is based on early French maps of the interior. Cognizance must be taken of the fact that, representation of details of the interior of the country on early maps is in the words of Warkentin and Ruggles following their detailed study of thousands of maps, both "spurious and incipient" (1970) hence a measure of conjecture is always involved in their interpretation. Map makers were usually unfamiliar with the country being mapped, hence they were working with second-hand information, which was provided by individuals who were involved in an internecine struggle for new territory, furs and souls. Consistent with this diversity of interest, information was jealously guarded hence wide cartographic differences exist even within one decade. In some cases, northwestern Ontario is represented by a bewildering confusion of lakes and rivers. The situation has often lead to misinterpretation of the location of Lake Nipigon, Winnipeg and the border lakes (Warkentin and Ruggles 1970; White 1914; Wilson 1910; Winchell 1911). These locations are salient to the consideration of which peoples were resident in the southern portion of northwestern Ontario at the time of contact.

It has been recently contended by Charles A. Bishop and others that, the Assiniboine at the time of contact occupied portions of northwestern Ontario and that they were resident in the area west from the Kaministikwia River at Thunder Bay, (Bishop 1974; Bishop and Ray 1974; Bishop and Smith 1975). The "Handbook of American Indians" earlier made a similar inference (Hodge 1912:45).

An initial important reference cited is to a single early mention of the Assiniboine beyond Lake Nipigon, reportedly recorded prior to the historic records of major population shifts, circa 1637, (Bishop and Smith 1975:57). There are no maps or records of Euro-Canadians visiting the area at this early date, thus at best the reference would have to be based on second-hand information. It occurs in the Jesuit Relations of 1656-58.

Here it is stated that the people living thirty-five leagues or thereabouts from Lake Alimibeg (Nipigon) are called the Nation of the Assinipoualak,(Assiniboine) or "Warriors of the rock", (Thwaites 1896-1901, 44:249) Apart from the nebulous nature of the reference, to conclude from a single report that a particular people resided in an area is spurious. On this basis a claim could be made for the early presence of Iroquois west of Lake Superior for they are so shown on a Dutch map of 1673 (Winchell 1911:26). The claim is even more tenuous when such turbid allusions to Assiniboine, as the reference of Gabriel Marset's report from the mouth of the Nelson River in 1695 of Assiniboine and Cree towards Lake Superior, is used to support the position, (Bishop and Smith 1975:57). Even a cursory examination of the early records of peoples in northern Ontario reveals a confusing array of names few of whom can be positively identified and virtually none of whom are consistently located in a specific local, (cf. Wright 1965).

An examination of French records shows that Etienne Brule visited Lake Superior as early as 1622 but it was only following the return of the French Colonies in 1632 that they actually sought out the Indians in their own territory on the rivers draining the north slope. This was by way of the Great Lakes system, (Ontario 1878:135). The earliest known map to show a portion of the area is Champlain's map of 1632 "Carte de la Nouvelle France". (op. cit.) It shows the country westward to the limits of discovery. This includes territory north of the eastern end of Lake Superior (Grand Lac) to approximately the Michipicoten River. On Nicolas Sanson's similar map of 1650 'L'Amerique Septentrionale", a number of the peoples of the area are shown. The Irini Nadous are shown to the northwest of the river and the Kiristinous are shown to the north, (Warkentin and Ruggles 1970:31).

Medard Chouart des Groseilleirs and Pierre Esprit Radisson penetrated the interior during their explorations in 1658-59, (Nute 1943) but it was not until Father Franciscus Creuxius (Du Creux) map of 1660 "Tabula Novae Franciae" that the interior between Lake Superior and James Bay is portrayed. Lake Nipigon (Lacus Alimibegoueci) is shown with the Nipigon River (Klistonum Fl.) draining south to Lake Superior. North of the lake are shown the Kilistones Alimibegoueci (Cree of Lake Nipigon). From an unnamed lake to the west a river, the Assinipoualacus Fl. is portrayed draining east into Lake Nipigon. This appears to be the first use on a map of the synonym for Assiniboine which gradually becomes synomymous with the major lakes of Manitoba (Warkentin and Ruggles 1970:12).

Father Allouez in the Jesuit Relations of 1666-67 reported that to the north and north-west of Lake Superior were several Indian Nations, one of which laying far beyond the Cree were called the Assinipoulac, (Thwaites 1896-1901, Vol. 51:57). He also describes a river (Nelson?) entering Hudson Bay used by the Assinipoualacs. Father Marquette in 1669 reported that he heard of a river(Nelson?) which flows into the western Ocean from the lands of the Assinipouars, who live to the west of Lake Superior on a lake (Winnipeg?) where they gather wild oats, (rice) and fish are plenty, (Thwaites 1896-1901, Vol. 54:193). The reference has to be to the lake complex at Winnipeg not Lake Nipigon, for it is north of Lake Superior not west and there is no great river flowing north out of the lake as it lies south of the height of land.

Father Dablon in the Jesuit Relations of 1670-71 states that the peoples called Assinipoulac were reported to live in one large village or thirty small villages in one group about two weeks journey west and north-west of Lake Superior, (Thwaites 1896-1901, Vol. 55:97-98). On the Jesuit Relations map of 1671 the western end of Lake Superior and the border lakes and rivers are depicted for the first time. Very significantly, there is an inscription on the north shore of Lake Superior opposite Isle Royale which states "R. par ou l'on va aux Assinipoualac a 120 lieues vers le nor ouest", (Thwaites 1896-1901; Vol. 55: facing 95). A similar note is affixed to Marquette's map of 1673 (Ontario 1878:136b). The reference appears to be from a location at the mouth of the Kaministikwia River, the route to the west used by Jacques de Noyon in 1688. Another map dated 1680 (Carte d'une grande partie du Canada, depuis Quebec jusq'au fond du Lac Superior...) has a similar note on the shore which reads "Par cette riviere on va aux Assinepoulacs a 150 lieues vers le Nord-Ouest ou il y a beaucoup de castors", (Ontario 1878:136b). The same note appears on Abbe Bernou's map of 1682, (Warkentin and Ruggles 1970:42). Franquelin's map of 1681 and Hennepin's map of 1683 place the Assiniboine around Lake Winnipeg. (Winchell 1911; Thwaites 1903).

It is clear from these references that the Assiniboine resided about 300 to 375 miles (1 lieue = 2 1/2 miles) to the northwest from a point on the north shore of Lake Superior about the Kaministikwia River. This places them at or beyond Lake of the Woods at the time of contact. Further, on the map of 1680 and on the Jesuit map of 1670-71, there is a note at the St. Louis River in Minnesota which reads "Par cette riviere on va au pays des Nadouessiens a 60 lieues au couchant..." Another early map ("Carte de l'Amerique Septentrionale contenant le pays du Canada ou la Nouvelle France la Louisiane...Acadie et Ile de Terreneuve en...l'aunee 1688 Par Jean Baptiste Louis Franquelin Hydrographe au Roi a Quebec en Canada") shows Lac Buade (Red or Leech Lake) situated northwest of Fond du Lac and a river (the Red) which runs almost due north to the Lac des Assinebouels, out of which a river (Nelson) runs to Port Bourbon or Hudson Bay. The country of the Assinibouels is shown north of the lake (Ontario 1878: 136c). The historical records then clearly show that the Assiniboine inhabited an area northwest of the boundary waters of northern Minnesota and Ontario at the time of contact.

Considerable emphasis is also placed on maps produced in 1685 and 1696 by the Jaillot family as supporting evidence for the presence of Assiniboine in northwestern Ontario. The reference given is Voorhis (1930) although he only provides one map reference. On these maps a post is shown on Lake Nipigon with a note which states that, it is the "Poste du Sr Duluth pour Empecher less Assiniboels et autres sauvages de descendre a la Baye de Hudson", (Bishop and Smith 1975). The post was constructed in 1684 by Daniel Greysolon, Sieur Dulhut under a policy laid down by the French in 1681 designed to deny the English furs by constructing posts on the interior rivers going to Hudson and James Bay. It was known by various names - Fort La Maune, Latourette, Outoulibis - and was geographically located by map makers north and generally west of Lake Superior. In fact, there may have been as many as three posts, (Nute 1943). Dulhut does not single out the Assiniboine. He provides a list of people to the west of the Bay commencing with the Cree and the Assiniboine on the plains, then the Algonkians of the Woods (people from the Sapiniere) and lastly, varying groups of Upper Ottawa Algonkians. In his report of 1685, he makes the following statement, "The Klistinos, the Assenepolacs, the people from the Sapiniere, the Openens, Dachiling, the Outoubouhys and the Tabitibis which comprise all the nations which are to the west of the Northern Sea, have promised to be, next spring, at the fort which I have constructed...", (Ontario 1878: 108).

Baron de Lahonton, an official military recorder who visited the Upper Great Lakes before 1700, shows the post on his map of 1703 west of Fort Nelson at the mouth of the Hayes River on Hudson Bay, (Thwaites 1905). The post appears to be located at Split Lake, a widening of a river (Nelson) coming from a village of the Assiniboines which is further west on the Lake of the Assiniboines. The French knew before 1684 that the Assiniboine lived on the plains, what appears evident is that, the geographic configuration of the interior country was still very much an unknown. The reference does not support the contention of Assiniboine resident in northwestern Ontario.

Further the proponents of the view have a tendency to overstate their case. It is claimed, for example, that the evidence strongly supports the view that the Assiniboine were the main occupants of the boundary waters area of northern Minnesota and Ontario during the 17th century (Bishop and Ray 1974:33). The prime evidence put forward is from Jenness. He is cited as stating that the Assiniboine inhabited the area between Lake Nipigon and Lake of the Woods. Jenness actually said that the Assiniboine hunted in the area (1963:308) which was controlled by the Ojibwa (ibid:277). This is a very different statement. Other examples could be cited but of greater interest is Jenness's source for the claim that the Assiniboine were present in the area. He refers to the Jesuit Relations of 1637-38 (ibid: 308 f.n.1). Here it is stated that "The Assinipouars...are westward from the Mission of St. Esprit, being fifteen or twenty days journey distant..." (Thwaites 1896-1901, 54:193). The mission is near La Pointe, Wisconsin (Thwaites 1896-1901, Vol. 55 map facing 95). Fifteen to twenty days journey westward would place the Assiniboine well beyond Lake of the Woods. Hlady's placement of the Assiniboine in Ontario at this early period (1964:10) is based on Jenness and Jesuit Relations references to Assiniboine thirty-five leagues beyond Lake Nipigon previously discussed The position is rejected as unsubstantiated.

The term "Assiniboine" means "Stone-Siou" (Barage 1878:52), Sioux being an Algonkian word for snake which, when applied to people, meant enemy, (Hodge 1912:326). Significantly, the word was applied in various forms by the Algonkian speakers to the Eskimo, Siouan and Iroquois peoples, (ibid:325). The term Assiniboine then appears to clearly identify an enemy people in a geographic region, that is, at the stones or shield. From the Hudson's Bay Company records comes further evidence that Assiniboine are foreign to the area for the Cree said that the Assiniboines were "people who did not know how to kill beaver," (circa 1734) (Rich: 1960:528). In other words, the Assiniboine were aliens to the area who were as Rich has said enticed into the trade, (op. cit:516).

This is not to say that the Assiniboine were not for some time involved in the fur trade on Lake Superior. Indeed, Lake Superior was occasionally referred to as the Grand Lac des Nadouessioux "La Nouvelle France dressee sur dernieres relations modernes, 1643" (Ontario 1878: 136) as was Lake of the Woods as late as 1690, "Mondo Nuovo" by Coronelli (op. cit: 136d) and the Assiniboine were represented at Sault Ste. Marie in 1671 when Sieur de Saint Lusson took possession at the northwest territory, "Countries of the Outaouas and of the Indian Nations of the North and West", on behalf of the French government (Jolliet's Article, op. cit: 62). There is no question that the Assiniboine were in northwestern Ontario before 1700, but they were there in the context of intercourse for the purpose of trade. Their presence in the area was very brief, for by the time La Verendry's party visited Lake Winnipeg before the mid 18th century, they recorded no such people east of the lake, (Moore 1927: 148,164).

All the historical reference record extensive groupings of Algonkian speaking peoples in northern Ontario. The earliest is Father Le Jeune's record of the location of Indians in 1640. He records the peoples who live on the shores of the Northern Sea as the Outimagami, the Ouchegami, the Mitchitamou, the Outhrbi and the Kilistinon, (Thwaites 1896-1901; Vol. 18:229). Speck's much later ethnological study of the Timiskaming Algonquian subdivision of the Ojibwa records various groups which appear to be the same peoples noted in the 17th century, (1915:3) These northern interior groups are considered by Bishop to be related to the Upper Ottawa Algonquians, (Bishop and Smith 1975: 56; 1975:198) which appears consistent with historical documented evidence.

Le Jeune's listing does not go beyond the eastern end of Lake Superior. Bishop, while recognizing that, at the time of writing nothing was known of the area to the west, considers this to delineate the extent of Ojibwa residence to the west citing Jesuit Claude Dablon's later reference to the Outchibous (Ojibwa) and Marameg resident at Sault Ste. Marie in 1670 as supporting evidence of the furthest extent of the area occupied by the Ojibwa, (Thwaites 1885-1905, Vol. 54: 131-135). He takes the absence of 17th century references to Ojibwa to the west to strongly indicate that there were no permanent Ojibwa groups west of the east end of Lake Superior before the 18th century. The premise seems to be based on the assumption that groups recorded in 17th century to the east were clearly defined and unequivocaly located. The historical records do not substantiate this view.

Bishop further places considerable emphasis on population shifts precipitated by the Iroquois wars and the fur trade, claiming that this has resulted in such mixing of groups that many original tribal group designations have been lost, (Bishop and Smith 1975: 56). It is suggested that high mobility characterizes the peoples of the Canadian Shield and that, the particular movement of peoples cited by Bishop has taken on importance because it has been recorded by Europeans. Boreal forest peoples moved of necessity in response to forest fires, availability of game and climatic fluctuations and this was facilitated by extensive kin-ties throughout the area. It was not a new phenomenon preceptated by the fur trade.

(Dawson 1975c; cf. Fisher 1969).

Marest, for example, when speaking of the Cree says that they have no villages or fixed dwellings, that they are wanderers and vagabonds living by hunting and fishing. Only in the summer do they assemble near lakes where they may remain for two or three months. He also notes that he has seen them at Michilimakinak and some as far east as Montreal, (Thwaites 1896-1901, Vol. 66: 107). Dablon and others make similar statements about unidentified peoples who lived in the northwest. (op. cit) The archaeological record supports the view of high mobility of Algonkian speaking peoples of the Boreal forest. (Dawson 1974, 1975a, 1975b; Wright n.d.) It is not surprising then that identity is confusing.

Dablon makes reference to many other Algonkian speaking peoples which go toward the north. Geographically, this would be along the Lake Superior shore for the lake extends about two hundred miles north from Sault Ste. Marie Archaeological recoveries from the Terminal Woodland period along the shore and as far north as Lake Nipigon reflect a uniformity of lithics associated with mixed ceramics from east, south, west and north,(Conway 1975; Dawson 1975a, 1975b, 1975c; Wright 1965, 1966, 1968). A similar situation pervails to the north-east, (Ridley 1954; 1966; Wright 1968c, n.d.). The situation is considered to reflect a continuous occupation by shifting groupings of Algonkian peoples, (Dawson 1975a).

Towards the west and north, various groupings have been considered divisions of the Cree which is the position taken by Bishop, however, many authors regard such people as offshoots of the Ojibwa, (Hayden 1862; Houston 1974; Warren 1957) and some authorities consider some of the divisions as Ojibwa, for example the Monsoni. (Hilger 1951:277; Morton 1939:11;Wright 1965:190) Archaeologically, the area reflects a mixture of Blackduck (Evans 1961; Wright 1966) and Selkirk (Hlady 1971) tradition recoveries which would be consistent with a mixed population of Ojibwa and Cree.

It would appear clear from listing given in the Jesuit Relations of 1658 that the various groupings of peoples then known as Kilistinons which is generally accepted as Cree, resided about Lake Nipigon, (Thwaites 1896-1901, Vol. 44: 249; f.n.: 325). However, Father Allouez's observations when visiting the Algonkian speaking Nipissings at Lake Nipigon in 1667 appears to be more characteristic of the situation. He notes twelve or fifteen apparently related groups coming from the north, south and east, (Thwaites 1896-1901, Vol. 50: 267). Archaeological recoveries from the area reflect in their ceramics this diversity, (Dawson 1975c). West of the Lake Nipigon to the border lakes region the same situation prevails although the ceramics characteristic of the eastern and southern affinities decline, (Dawson 1974). The manifestations show no discontinuities suggestive of intrusions of diverse peoples which should be the case if Assiniboine peoples were resident in the area. This statement is predicated on the assumption that the Assiniboine presence would be evident by the presence of Mississippian ceramics.

In the border lakes area, the Monsoni appear to be in command of the territory in 1663 at the time when Ojibwa in Grand Council made peace with the Sioux at the western end of Lake Superior, (Hlady 1964:14) At the same time, the Cree appear to be in command of Lake of the Woods. (op. cit.) Later in 1733 La Verendry records eight hundred warriors of Monsonis and Cree at Lake of the Woods, (Moore 1927:134). This appears to clearly indicate that the Algonkian speakers not Assiniboine Souian peoples were resident of the area. Archaeologically, this area is dominated by the Blackduck tradition and it is suggested that it was the product of the Monsonis-Ojibwa. Bishop in support of his thesis, rejects the considered opinion, (up sup.) that Monsonis are part of the Ojibwa group and equates them with the Nipissing Cree recorded in the Jesuit Relations of 1656-58 as residing between Lake Nipigon and the Moose River, (Bishop and Smith 1975: 56).

Another Algonkian-speaking people are in the area of Rainy Lake and south in the 17th century (circa 1680) and they are the Cheyenne who appear to have been associated with the Monsoni, (Hodges 1912:251; Grinnell 1923). These two groups could account for the long time depth (circa 1000 years) of the dominance of the Blackduck tradition which continues unbroken into historic times in the border lakes area.

The unbroken archaeological record does not support the view of a recent intrusion of an alien peoples, nor does a review of the historical record which clearly places the Assiniboine around Lake Winnipeg with only transitory appearances in Ontario in the context of intercourse for the purpose of trade.

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# THE CHILD WHO WAS NOT BORN NATURALLY—NECESSITY AND CONFUSION IN A CREE MYTH

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One of the useful properties of myths is that they take the form of stories. We can probably safely assume that when a narrator begins a myth, he and his audience understand that he is beginning a story. We can also presume that they understand when the story has ended. Thus, a myth is a bounded unit for analysis, a piece of culture, so to speak, that can stand by itself. Its boundaries are defined by the properties of a narrated story--it must have a beginning and an end which are more than just arbitrary breaks in the continuity of the narrative. It is true that a myth can exist as many variants or can be a story within a story, but this is a matter of context for the myth remains a bounded unit. All of the so-called variants, all of the myths in a sequence or cycle of myths, are related to one another yet each can stand alone, a nucleus in relation to which all other myths stand as extensions, variations, or expostulations of contextual understandings not explicitly stated in the myth itself. In other words, there always remains more to be said about whatever it is that a particular myth is saying but this does not imply that the myth is incomplete. Rather, it indicates that myths are productive and what they produce are always other myths.

In this paper I have taken a single Cree myth, The Child Who was not Born Naturally[1], as a bounded unit for analysis. No doubt this particular story has been retold countless times and we have evidence of this in the literature[2]. A preliminary analysis suggests that all recorded variants of this could by described as having been generated from the narrative which I have analyzed. The same could be said, of course, using any one of the variants as a starting point and so my choice of this particular one is arbitrary.

The myth in all of its detail is too long to reproduce here. What follows, therefore, is my own variant, a condensed version of the original myth.

The narrative begins with a husband, his pregnant wife, and their infant son. The wife is lazy and the husband hints that something (dangerous) may come into the camp. He instructs his wife that if this happens she must hide their son. While the husband is away hunting, an atoosh (a cannibal) comes. The son, who has been hidden, escapes unharmed but his mother is disembowled and eaten. The atoosh tosses away the womb containing the unborn child and is duly killed by the husband when he returns. However, unbeknownst to the husband, the womb with its child is discovered by mice who take the baby and raise it themselves. They steal food and toys for the boy but keep the relationship secret. During this time the father hunts, as he must, but he also sets up housekeeping with a lynx and fathers lynx children. This he keeps secret from his young son who is forbidden to travel in the direction that the father leaves the camp each day. The boy eventually discovers his lost brother because he becomes curious about why his toys keep disappearing. For a while he visits his brother secretly, but eventually the mice tell the boy to take his baby brother back to his father. All of the humans are then reunited. However, out of curiosity the boys eventually follow their father's tracks and discover the lynx family. They kill the female and her offspring and return with one of the young which they present to their father. He is dismayed and leaves at once. The boys then realize that they have killed their brother. Younger brother reasons that father will return to kill them and so they are prepared when they are attacked by the father and an army of lynxes. They succeed in killing all of the lynxes and their father. Now the boys are on their own together. Things seem to be going smoothly for a while until the older brother tries to establish relationships with a frog. Younger brother is disgusted and tosses the creature out of the tent. His older brother then succeeds in finding two wives for himself, for younger brother is uninterested in a human wife. When the younger wife tries to force her attentions upon the youngest brother, he leaves in search of an animal wife. In his travels, he has abortive affairs with a beaver, a caribou, a porcupine and a whiskeyjack, each of which he tries to live with but then leaves them because they are in some way unsatisfactory--beaver has black rotten teeth, porcupine is covered with quills, etc. He then seeks out the beaver again. She agrees to live with him as a human under the restriction that she never be allowed to touch a body of water. Out of sheer whim, however, younger brother violates this restriction and she transforms back into a beaver and swims away. Eventually he finds her again and she asks him to come live with her in the water. He then discards his clothing and becomes a beaver. He goes on to teach beavers to make better houses and to construct multiple entrances to these (something he learned as a mouse) and this endeavour is so successful that men begin to go hungry because they can no longer kill beavers. His older brother then seeks him out for he knows that he is responsible. The beaver lodge is torn open, his beaver wife killed and he is brought back to live with men. This he does but with the restriction that he cannot eat female beaver. Out of anger and curiosity, his older brother feeds him this forbidden food, whereupon he instantly changes into a giant beaver (a beaver atoosh) and swims away into the sea.

This particular myth combines in a striking way two contradictory qualities--it is at once simple and complex. Although the myth's exceptional richness and complexity is immediately apparent there is also a more obscure, contrary impression of simplicity or redundancy. This analysis is an attempt to articulate this redundancy, to develop a method for talking about it. The analysis is structural because it appears to me that it is not the myth's content that is simple or redundant. I assume, therefore, that the redundancy must be relational. Thus, I assume that the myth has a structure that is obscured by the complexity of its contents.

Since I have begun with what I call "the impression of redundancy" I should explain briefly what I mean by this assertion. First, from the initial warning of the husband the story takes on the air of something predetermined or necessary. This feeling is conveyed not only to me but to others who have read the story yet how the myth conveys this is by no means clear. Second, the end of the myth appears to be an inversion of the beginning--a human atoosh initiates the drama while an animal atoosh ends it; younger brother is torn violently from his mother's womb and then in the end from the beaver house (he is born unnaturally twice). Third, the actions of most of the actors in the myth, especially younger brother, appear confused, misinformed, absurd and hopeless, regardless of what it is that they try to do.

Viewed in this manner, then, the myth is not only redundant but the redundancy has a locus. It is concentrated in the body of the narrative between the opening and closing episodes, the segments of the narrative involving an atoosh.

Following Levi-Strauss (1963), I have assumed that a myth is an attempt by men to explain certain necessary but paradoxical relationships by demonstrating what might happen if these were transformed into logically equivalent, though different sets of paradoxes. The myth analyzed here begins with what I shall call normal necessary relationships; these are transformed into something contrastive through the agency of an atoosh; this contrastive condition is clearly labelled as disruptive and finally is transformed back into necessity through the agency of another atoosh. The redundancy within the body of the myth serves to emphasize the contrast of necessary relationships with indeterminate ones. The task of analysis becomes to develop a language for pointing to and describing this contrast for in doing so, the redundancy can be made explicit.

Let me make it clear at this point that I see structural analysis as a species of language, a method for talking about certain relational properties of myths. As such, its scope is limited, its explanitory power far from exhaustive. We must remember that any method sets out to discover what it assumes to be there; in this case relations that are contrastive, logically interdependent within a closed system and therefore, self-perpetuating. The structural analysis of myth presupposes that myths are redundant and that this is a derived property of their structure. In this sense, then, it is a method for mapping the redundancies of myths.

The analysis that I present here is a tentative one and not highly formalized. One property of a structural description is that it lends itself to formalization but this all too often creates the false impression of completeness, stasis, closure. Here I wish to emphasize the tentative nature of this description, its status as something emerging but not yet completed. Thus, I have eschewed for the time being the path of formalization.

I have chosen to present this analysis because it tells me something about what this peculiar story is about that is not at all obvious at first glance. A complex and bizzare story becomes an exploration into the relationship between _form_ and _content_, _continuity_ and _discontinuity_ by demonstrating what would happen if these became unlinked. Through the agency of at _atoosh_, necessity is abandoned to chance, whim, and personal competence but what results is not chaos but rather a structured species of confusion called the _double bind_ (Bateson 1972). In a sense, then, the myth deals not only with ordered and disordered _relationships_ but with _the nature of order and disorder themselves_. I am not aware that Cree culture is often portrayed as concerned with such matters--the Cree don't convey the offhand impression of orderly people--yet, if this is but one of many myths of a similar structure, then it would seem that the conventional view of this culture is greatly in error. If, as this analysis suggests, the collapse of necessity is equated with a specific kind of closed, self-perpetuating confusion, then we can imply that Cree culture is ordered by a _necessary_ logic that is especially susceptible to this kind of disorder.

## The Determinate Structure

In doing a structural analysis, a fundamental problem is finding adequate labels to mark the relational contrasts that emerge out of the narrative. In a very real sense it is the naming of these polarities that gives the structure an objective existence and this is the first step towards any formized description. What follows is the simplest statement of the myths structure--a system of two interdependent axes of opposition which I have called form/content and incorporation/exclusion. There are numerous other oppositions that can be identified but all appear to be transformations of these two fundamental ones. Although this structure became evident only after I had discovered the double bind, I have chosen to describe it first in order to better illustrate the peculiar confusion generated in the body of the myth.

In the world given to us by our senses, matter is largely in the form of things, objects. Since these exist in a context that we interpret as three-dimensional, things have form and content. We can say that form encapsulates content remembering that form is not separate from, but only an aspect of content; in other words, one defines the other. In a three-dimensional world form is the surface of content, therefore, form and content are necessarily isomporphic.

There is another way of viewing this paradoxical relationship. Form and content can be seen as products of two different points of view: the inside and the outside, or the subject and its object(s). From the viewpoint of the subject, content is necessarily hidden by form (there are only surfaces of objects). Content, then, is entirely the inside view of the subject. Therefore, form and content require the exclusion of subject from object yet we know that from the viewpoint of the object, the subject becomes object and thus form. Therefore, the exclusion of form from content demands a concomitant incorporation of the two. Thus we have a closed system of two pairs of paradoxical, mutually interdependent contrasts. The relationships within the system are determinate, necessary--one cannot be simultaneously the perceiver and the perceived for form and content have to be isomorphic. In simple descriptive terms, there is no possibility for the form of a frog to encapsulate the contents of a moose just as one cannot occupy the subjective position of a human while incorporating the objective form of a grasshopper. If objects were put together in this manner the material world would become ungluded and that is what this myth is all about.

All that remains to be said about this structure is that it applies not only to substances but also to behaviour. Behaviour is to experience as form is to content. Behaviour is the surface of feeling, the object and the subject's mask. Thus all that has been said of the substantive world applies to behaviour as well.

## The Double Bind

As the myth opens, the structure is intact. We have a human family, the father hunts, the mother is pregnant, humans are unambiguously human and animals apparently unambiguously animal. The scene is flawed by the wife's laziness but flaws such as this do not threaten the necessary order of things (or do they?). Yet the husband is afraid and his worst fears are realized. An atoosh, a human cannibal, sets in motion the chain of events that will lead to the disassociation of necessary relationships within the structure of things. He eats the mother, discards the womb with its contents and unknowingly abandons the human child to chance. After all, it is entirely by chance that the unborn child is discovered and nurtured by mice. In the determinate world such a relationship could not possibly happen. Thus, it is precisely at this point in the narrative that determinism is shattered and the world begins to come unglued.

Here we have younger brother (remember that he has an older human brother) incorporated into the society of mice. Having known no humans at all, he would naturally identify himself with mice--he is given no other choice for he is secluded from human society. We can assume that there is no confusion on the part of the mice. They steal toys and food for the boy, implying that they recognize his human status and treat him as a human child. From the boy's point of view, however, things are terribly array. Clearly he cannot understand the distinction between mice and men. Since he is the subject (human) and they are objects (mice) he does not even recognize the necessary distinction between subject and object. If we look at the total mouse-boy society we see that the boy has human form but mouse content (his subjective state). Form and content are therefore no longer isomorphic. We see also that he tries to incorporate the form of an object (mouse) as his own content without a concomitant exclusion of his human content as mouse form. Thus, the relationship between incorporation and exclusion is no longer determinate.

How the boy sees himself and others is utterly different from how others view him but this is totally hidden from everybody concerned because he looks and acts as a human being while thinking that he is something else. He is, in fact, a living impossibility, both a mouse and a human simultaneously though he cannot possibly understand this for he sees himself as a mouse just as the others see him as human.

When he is returned to his human family, he has no great difficulty. He relates well enough to humans, as well he should since he was reared like a human but in the company of mice. The fact, so obvious to us, that mice and humans do not look alike, has no significance to him at all for he has no reason to assume that form and content are isomorphic. Humans respond to him as if he were human and he responds to them as if they were mice yet *nobody can see that anything is wrong*. All are locked into a self-perpetuating delusion. This then is the double bind--that no matter what any of the actors do, it cannot help but be misunderstood by the others, their responses in turn misunderstood, etc. and none can see that this is what is happening. The necessary relationships of form and content, inclusion and exclusion have collapsed yet this is invisible to everybody concerned, regardless of their points of view. The focus of this inward spiraling, collective delusion is younger brother but since his ambivalence is invisible it spreads to all who enter the system.

From the moment that younger brother is discovered the double-bind becomes self-generating and the narrative therefore becomes redundant as it must. A double-bind can generate an infinite set of concrete situations, each of which would appear different, all of which would be collective delusions of precisely the same nature. Here then is the simultaneous complexity and simplicity of the myth.

I have discussed the initial situation of younger brother. However, inspection of the myth reveals another locus of disassociation in the opening segment. Disassociation within an individual (younger brother) is paralleled by disassociation with a social unit, the human family. The atoosh tears the mother out of the social fabric of the family just as he tears the younger brother out of his mother's womb. We are left with a lone child and a lone father, each of whom spends most of his time separate from the other. The unifying bond of the female--wife to her husband, mother to her children, procreator --is absent and necessity (for these family relationships are necessary relationships) is again abandoned to chance, whim, and personal competence. And what does the father do but take up residence with a lynx and procreate lynx children. By doing so he further separates himself from his growing son and creates another double-bind. His actions link two separate families in which form and substance are not isomorphic and which simultaneously incorporate and exclude each other. For example, a human father has both lynx and human children. A lynx is mother to humans and lynxes. Lynxes and humans are siblings yet cannot know this. Father's attempt to keep the two families physically separated is not determinate and inevitably fails. Not only does the apparently irrational travel restriction that the father imposes on his sons lead to his undoing but the very fact that he has kept his lynx family a secret makes the dual nature of the lynxes invisible to the boys. Thus, they murder members of their own family without knowing what they have done. Recognition of the act generates a hopeless wall of fear and anger, from any of the participants point of view. The only possible resolution is the annihilation of one of the families. Structurally, it makes no difference which one, but since this is a story about younger brother, it is the lynxes which are destroyed. Note, however, that this destruction requires the murder of the father by his own children so that the reincorporation of the human family is accomplished by an act that within the family can be only excluding. And the murders still does no good for the double-bind lies buried in another context and the family is incomplete for it lacks the necessary unification supplied by the female sex.

The narrative now turns to the boys' futile attempts to recreate a family. Remember that both boys are without a father or mother, one has grown up with mice and both have seen their father marry a lynx. It is perhaps not unexpected that in this confusion older brother tries to establish a relationship with a dangerous animal--a frog[3]. However, younger brother is disgusted by this (he recognizes the danger) and destroys the offending animal. Here is at least a hint that out of his peculiar confusion younger brother derives certain powers of discrimination not given to normal men. Despite his reaction to the frog, younger brother expresses total disinterest in human females. He desires, instead, a wife like his father took--some kind of animal. He acquires two women (sisters) for his older brother but when one of these tries to force her attentions upon him, he leaves in disgust to find an animal wife. In succession, he has abortive affairs with a beaver, a caribou, a porcupine and a whiskeyjack. All of these fail because he judges the appearance of his animal wives by human standards--beaver is messy and has black teeth, caribou has long legs and is obscene when she stands up, porcupine has a short snout and is covered with spines, etc. After this, he returns to the beaver. She transforms herself into a human this time but this can be maintained only be the restriction that she never touch the surface of a body of water. Thus, her exclusion from beavers and inclusion with men is dependent on an indeterminate restriction. Eventually younger brother violates the restriction on a whim, his wife immediately is transformed back into a beaver and swims away. In the end, he forms a stable relationship with this beaver only by entering the water to live with her and becoming a beaver. Here he makes positive contributions to beaver society derived from his childhood experience with mice. These effectively separate men from beavers, so much so that men have to go hungry and although transformed and separated, younger brother is still in a double-bind for he has a human brother. It is in fact his older brother who kills his beaver wife, tears him from his beaver lodge, and attempts to reincorporate him into the society of men. This can only mean that he looks and acts like a human, but remember, he has been transformed into a beaver and this has not been reversed. His odd food tabu, he must not eat female beaver, tells us what has happened--his content is beaver, his form human. But his brother cannot possibly know this for it is invisible to him. Out of curiosity and anger his brother feeds him female beaver (his wife) for remember that the difference between male and female meat is an invisible one (it is content and not form). Younger brother is immediately transformed into a giant beaver (atoosh) which escapes into the sea, therefore leaving the boundaries of the structured world. The act of eating is therefore defined as cannibalism and the relationship between cannibalism and the atoosh is a determinate one. It is the atoosh that tries to incorporate its own form as content, an impossibility indicated by the fact that the atoosh swells up and becomes monstrous. However, the impossibility of the atoosh is visible from any point of view and therefore cannot generate a double bind. Thus, what the myth has done is to resolve the double bind in the only way that it can be resolved: by redefining it within another outside frame of reference. In other words, it is made visible and therefore excluded in the form of a monster. In the end, we discover that the confusion, though disruptive and tragic, does not spread indefinitely. Sooner or later, even if left to chance, it will encounter something that is determinate within any frame of reference and at that point will be revealed as a logical impossibility.

## Conclusions

I have endeavoured to show that the redundancy in this myth can be described in terms of a structure. In doing this, I have also been able to say something about what this myth is about that is not immediately obvious. To my knowledge, nobody has said this before. In this sense of adding something new, I have not reduced the myth to some abstraction but rather, enriched it by showing that it is more than we thought that it was.

There are implications, generated by this analysis that extend beyond the narrative itself, to include not only other related narratives but totally different aspects of Cree culture. The structure of this myth points to other things, other relationships that might well be re-examined within a different framework.

For example, I have often heard it said that for the Cree, appearances can never be trusted; things cannot be relied upon to be what they appear to be. Now, if we take this myth as a guide, then we see that for the Cree, like ourselves, things *must* be what they seem--that is the only determinate logical way in which the world can be put together. This at least is what we place reliance upon, but there is always the possibility that we might be mistaken. And here is where the myth makes an extraordinary statement--if we are mistaken and things can be other than what they seem, we cannot know that this is the case for it would be invisible to us. All that we would see would be the intrusion into the world of misguided actions, poor judgment, inexplicable changes in mood, misfortune, tragedy, etc. Now all of these are the constant companions of human life and raise the spectre that determinacy just might be always coming apart, necessity unravelling into the snare of dilusion. The message is clear--there is a real danger that this could happen if chance intrudes into the affairs of men and animals. We must remember that the double bind is the result of an erroneous point of view that cannot be revealed as such and that it feeds upon determinancy at a locus that lies outside of shared understandings. What then, are the safeguards that can prevent the spread of delusion or reveal it to the eyes of men. I can think of at least one immediately--the atoosh, the anti-person who is a monstrosity from any point of view and thus outside of the realm of falsification. The atoosh when it appears, the atoosh when men fear its appearance, brings the assurance that misfortune it brings is not the surface of hidden delusion. Remember that the atoosh does not discriminate, meaning that the species of confusion expressed in the double bind is irrelevant as far as it is concerned. Therefore, the atoosh occupies a fixed position unaffected by one's point of view. It is confused, horrible but visible. There is the other side, of course. The atoosh is terrifying and monstrous as it must be but these very qualities, generated by its indiscriminateness and lack of control, are a threat insofar as they bring separation and chance into the world. Thus, the atoosh is bred of impossibility but also creates it. The myth seems very clear on this point. I am intrigued by the suggestion derived from this myth that the Cree cosmos is populated by a species of monster as a safeguard against the possibility of things becoming other than what they seem.

There is something else of great interest implied by the structure of this myth. In this world of things given to us by our perceptions and experience, there is no single outside or objective point of view. The double bind is a disease of points of view as the myth makes clear. It requires at least two independent points of view as a necessary precondition and this is given by the elementary structure of the myth itself. There is always a subject and thus always an object and thus relationships are necessarily reciprocal. Now we are accustomed to thinking that there is a cosmos that lies external to and not imbedded in a particular point of view. In Indo-European mythologies the cosmos is created by the gods and therefore object. But is it not possible that for the Cree there is not any single objective cosmos in this unambiguous sense? This myth, and many others strongly suggest that what is considered as objective knowledge is some sort of agreement among reciprocal subject-objects which overcomes the discontinuity inherent in their independent points of view while not falsifying them. In other words, there are men and there are mice and there are agreements between the two that define some object common to both. But for men mice are objects of nature and from mice it is men who are. Again and again Cree myths imply that animals have social lives like men but these are hidden and only implied by what they do, what men can observe. However, the world is not fragmented for men and the various animals relate in regular ways which define some overall scheme as symbolized in the Cree assigning kinship terms to the animals with which they share the earth. In other words, the world order is egocentric and much like a cosmic kinship system. As I stated earlier, such a system is suspectable to the disease called double bind whereas a uniformly objective cosmos is not. It is vitally necessary that no person ever be put in a context where he might be deluded into occupying two simultaneous points of view but this is nevertheless a real possibility. One safeguard, however, is to stabilize contexts so that this just cannot happen: to mark as overtly and as carefully as possible all relationships that could lead to mistaken viewpoints. Thus, although men and beavers engage in a definite relationship, they share some segment of reality in a special, specific manner that differs from, say, the relationship of man to bears. It must be made clear that this shared reality does not unite beavers and men or bears and men as a single species. Is it possible that the Crees meticulous ritualized concern with hunting and eating, two classes of activities that are pervasive, necessary and also relational in the above sense is an effort to state interdependency without the ambiguity which is such a danger in their cosmos? There is the suggestion here of some carefully constructed order which stands in great contrast to the apparent flux and plasticity of Cree social life.

## Footnotes

1. This narrative was collected by Richard Preston at Rupert House, Quebec in January, 1964.

2. I know of at least two recorded variants of this myth; Bell (1897) and Bauer (1971).

3. The Cree apparently regard frogs and toads as potentially dangerous beings because of their ability to transform their appearance. In this case, the danger was that the frog, if it slept with older brother, would be able to transform itself into a woman.

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# UNSPECIFIED-SUBJECT PHENOMENA IN ALGONQUIAN

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## 0. INTRODUCTION[1]

A number of surface morphological and syntactic facts in languages reflect the semantic non-specification of a logical subject for underlying predicates. The purposes of this paper are to pull together such facts from representative Algonquian languages, attempt to characterize both typical and atypical Algonquian treatment of unspecified logical subjects, and to discuss theoretically controversial analyses of these phenomena such as the claim that certain forms are or are not "passives". In addition, I hope to stimulate other Algonquianists to look for syntactic evidence for their analyses.

I shall first (1.) discuss structures involving predicates which allow no logical subject ("impersonal" verbs), then (2.) intransitive predicates with subject unspecified, then (3.) transitive predicates with subject unspecified. The final section (4.) will discuss theoretical treatment of these phenomena.

## 1. IMPERSONAL VERBS

These reflect underlying predicates which take no logical subject. They never have plural inflection, and where the languages distinguish AI inflection from II, such verbs have the singular II affix.[2]

Examples:

| | | |
|---|---|---|
| Menomini: | *keqsiw* | 'it is cold' (Bloomfield 1962:45) |
| Kickapoo: | *weepenaawi* | 'it rained' (Voorhis 1974:54) |
| Plains Cree: | *ki:sika:w* | 'it is day' (Wolfart 1973:38) |
| Blackfoot: | *aoxpottawa* | 'it is snowing' |
| Micmac: | *wicu:saq* | 'it is windy' |

Bloomfield (1962:45) says that such verbs are II in Menomini, though in the singular there is no difference between third person animate and inanimate subject inflection; this is true in Blackfoot and seems to be true in Cree as well. (Bloomfield was probably influenced by Fox which has -*wa* for third person animate subjects but -*wi* for inanimate subjects, and so impersonal verbs are clearly II in that language, as in Kickapoo.) And of course the fact that impersonal verbs cannot be inflected for other than third person also places them in a class with II verbs.

We should not be content to rely upon strictly morphological evidence to decide such questions, however. If there are syntactic processes in the language which are sensitive to the categories at issue, in this case whether a verb has an animate or inanimate syntactic subject, they can serve as tests to decide the question. Blackfoot, for example, exhibits a process of copy-raising from complements (Frantz - in press), as exemplified in (2), which is synonymous with (1).

(1) nits-ikkstaa m-aaxk-sooyi-xsi
1- want(AI) 3-might-eat-conj
'I want him to eat'

(2) nits-iksstat-awa m-aaxk-sooyi-xsi

1- want(TA)-3 3-might-eat-conj

The only difference between (1) and (2) is that the matrix verb 'want' of (1) is inflectionally intransitive, while that of (2) is transitive animate(TA), having as its object the third person which is also subject of the complement verb 'eat'. Thus a copy of the complement subject has been added as object of the matrix as part of the derivation of (2).

Now, by putting an impersonal verb in a complement, we can determine whether the impersonal verb has an animate or inanimate subject on the basis of whether copy-raising produces a TA or TI (transitive inanimate) matrix verb. And we find that (4), which is synonymous with (3), has a TI verb. Thus the complement verb 'snow' must have third person inanimate as a subject, in order to account for the inanimate copy in (4).

(3) nits-iksstaa m-aaxk-oxpotta-xsi

1- want(AI) 3-might-snow-conj

(4) nits-iksstatoo-xpa m-aaxk-oxpotta-xsi

1- want(TI)-3

(We can rule out the possibility that the entire complement of 'want' is being created as an inanimate object, because we see in (5) that complements of 'want' cannot themselves trigger verb agreement:

(5) *nits-iksstatoo-xpa m-aaxk-sooyi-xsi

1 -want(TI)-3 3-might-eat-conj.

## 2. INTRANSITIVE PREDICATES WITH UNSPECIFIED SUBJECTS

Every language provides a way for speakers to avoid specifying the logical subject of a predicate, though of course the very use of that predicate presupposes that it has a logical subject, whether specified or not. In the case of intransitive predicates with subject unspecified, we find that there is not uniformity among the Algonquian languages.

According to Rogers (1960) the resultant verbs are II in Mistassini: *pi:hto-na:niw* 'there is smoking' (Rogers 1960:107). Thus the suffix *-na:niw* in Mistassini is derivational in that it derives II stems from AI stems. This is clearer in the conjunct, where third person is marked differently for II and for AI: *e-pi:htona:niw-c* 'that there is smoking'; cf. *epiihto-t* 'that he is smoking'.

Plains Cree uses a similar derivational suffix *-na:niwi* in the independent order (*pici-na:niwi-w* 'camp is moved' [Wolfart 1973:62]), but not in the conjunct. Wolfart assigns the conjunct forms to the AI paradigm: *e:-wi:h-pici-hk* 'when camp is moved' (p.62). However, if one were to segment an *-h* as the conjunct allomorph of *-na:niwi*, what remains in such forms is the regular II suffix. Here we need more than inflectional evidence. I suggest that research needs to be directed into the search for syntactic evidence to decide whether these "indefinite subject" forms are II or not in Plains Cree.

Menomini has suffix *-n* in the independent order and *-h* in the conjunct, where Delaware has *-n* and *-nk*. Neither Bloomfield (1962) nor Goddard (1969) say that these verbs are II; I think they intend the reader to understand that they are AI. But here again there is a need to look for possible syntactic evidence that these verbs are superficially II rather than simply subjectless AI verbs.

Kickapoo (Voorhis 1974) has a suffix -pi in the independent, and -ek in the conjunct: iihwiiθenipi 'one will eat' (p.61), eehpenoki 'when one went home' (p.79). Here it is at least possible that the i of -pi and the k of -ek are the regular II suffixes. But again one should look for syntactic evidence either way.

In Blackfoot, unspecified subject forms of AI (and TI) verbs are homophonous with the 12 ('we' inclusive) forms of those verbs, i.e. have suffix -o'p; cf. aipasskaa-wa 'he's dancing' and aipasskaa-o'pa 'there's dancing'/'we(12) are dancing'. And as far as I am able to determine, no II stem ever occurs with this suffix; AI stems may be used without implying that the unspecified subject is animate.

Now, to practice what I preach, we should look for evidence for or against the unspecified subject form being superficially II as seemed to be the case in Mistassini. As with the impersonal verbs of 1., we can put one of the verbs which are in question here into a complement and observe whether subject copy-raising is applicable to give a TI matrix verb. Thus we first look at (6), which is ambiguous as shown, and if the complement with the unspecified subject meaning is syntactically II, then (7) should be possible. However, as the asterisk indicates, (7) is not an acceptable Blackfoot sentence. (Incidentally, (8) is acceptable and unambiguous as shown.)

(6) iiksstaa-wa aaxk-aipasskaa-o'si
want-3 might- dance-unspec/12
'he wants { there to be dancing
us (12) to dance '

(7) *iiksstatoo-xpa aaxk-aipasskaa-o'si
want(TI) -3 might-dance-unspec(conj)

(8) iiksstak-kiwa aaxk-aipasskaa-o'si
want(TA)-12 3 might-dance-12(conj)
'he wants us(12) to dance'

Thus as far as this particular test is concerned, unspecified subject forms of AI verbs are not superficially II as they seem to be in Mistassini and may be in the other Algonquian languages polled.

Finally, Micmac uses periphrasis to avoid AI subject specification: *amalkewaqan* *pemiaq* 'dancing is going on.'

3. Lack of specification of the subject of a transitive predicate involves the most interesting and most controversial forms. I will attempt to avoid theoretical discussion until the next section, here summarizing what various investigators have reported.

Looking first at TI verbs, most of the languages treat these identically to AI verbs in regard to lack of subject specification, as we might expect since AI and TI inflectional paradigms are the same or very nearly the same in most Algonquian languages. Of the languages mentioned in 2., my cursory check indicates that only Delaware, Mistassini, and Micmac TI verbs need to be dealt with separately from AI verbs in this section. Delaware unspecified subject TI verbs are inflected the same as AI unspecified subject verbs in the conjunct, but only rarely so in the independent, where they usually form a "derived passive" with suffix -*a:si* (Goddard 1969:133,4). According to Rogers (1960), Mistassini adds a suffix -kani to TI verbs, and the result is an II verb. The Micmac unspecified subject forms for a TI verb appear to be a potpourri of forms from other paradigms, including three from the corresponding TA forms, three from the regular TA paradigm, and three apparently unique suffixes.

Of more theoretical interest is the question of whether these TI verbs with unspecified subject are inflectionally II because of the near identity of AI and TI paradigms, or because the logical object of the TI verb has become the syntactic subject of these verbs. This we will touch on in 4.

Turning now to the TA verb, we find that just about all the languages have unspecified subject paradigms which can be analyzed as the usual AI affixes attached to the TA stem plus a formative. The languages differ in the number of variants this formative has in the paradigm. In Micmac it is constant through the paradigm (-uksi) while in Blackfoot it has three forms (-ot, -okoo, and -a). In most of the languages we find but two; e.g. Cree has -ikawi and -aː. Compare these three paradigms:

| logical object | Micmac 'carry' | Plains Cree 'see' | Blackfoot 'bite' |
|---|---|---|---|
| 1 | pemaːl-uksi-Ø | ni-wapm-ikawi-n | nit-ssiksip-okoo |
| 1pl | pemaːl-uksi-yek | ni-wapm-ikawi-naːn | nit-ssiksip-ot-ixpinnaan |
| 2 | pemaːl-uksi-n | ki-wapm-ikawi-n | kit-ssiksip-okoo |
| 2pl | pemaːl-uksi-yok | ki-wapm-ikawi-naːwaːw | kit-ssiksip-ot-ixpoaawa |
| 12 | pemaːl-uksi-ikw | ki-wapm-ikawi-naːnaw | isiksip-ot-ixpa |
| 3 | pemaːl-uksi-t | wapm-aː-w | isiksip-a-wa |
| 3pl | pemaːl-uksi-icik | wapm-aː-wak | isiksip-a-yawa |
| 4 | pemaːl-uksi-initːl | wapm-im-aː-wa | isiksip-a-yini |
| 4pl | [same as 3pl] | [same as 4] | isiksip-a-yi |

Compare the corresponding AI affixes:

| subject | M | C | | B | |
|---|---|---|---|---|---|
| 1 | -0 | ni- | -n | nit- | |
| 1pl | -yek | ni- | -na:n | nit- | -ixpinnaan |
| 2 | -n | ki- | -n | kit- | |
| 2pl | -yok | ki- | -na:wa:w | kit- | ixpoaawa |
| 12 | -ikw | ki- | -na:naw | | -o'pa |
| 3 | -t | | -w | | -wa |
| 3pl | -icik | | -wak | | -yawa |
| 4 | -initil | | -yiwa | | -yini |
| 4pl | [same as 3] | | [same as 4] | | -yi |

Observe that -uksi runs throughout the Micmac unspecified subject paradigm, and the suffixes which follow are identical to the AI set below. Hence there would be little reason to question an analysis which said that -uksi derives AI stems from TA stems. In Plains Cree, however, things are not so straightforward. An analysis such as that just stated for Micmac is complicated for Cree by the necessity to say that there are two forms of the derivational suffix (-ikawi and -a:), and furthermore that the suffix for 4th person is different from that found in the AI paradigm. These facts, plus the identity of shape of this a: with the direct theme sign of the TA paradigm, led Wolfart to say that the non-third portion of this paradigm is a separate paradigm, and that the remainder belongs to the basic TA paradigm (Wolfart 1973:62). Finally, Blackfoot, has three different formatives after the stem, and further complicates the analysis in that the 12 suffix is not identical to that of the AI paradigm.

But here again we should look for syntactic evidence. Wolfart's decision to split this paradigm is fine if we are concerned only about morphological inflection. But if this split has validity within the grammatical system of the language, we expect to find syntactic evidence for it as well. We will discuss this further in 4.

Most of the other Algonquian languages I have checked follow roughly the Plains Cree pattern in having two variants of a formative added to the TA stem and adding to this the regular AI affixes. Fox and Kickapoo additionally have a p (apparently the reflex of Goddard's *hm [1974:322]) throughout this paradigm for independent verbs; the corresponding forms in the conjunct have a distinctive morphological character (Voorhis 1974:82-83).

4. It is virtually impossible to discuss analysis of these phenomena without assuming some sort of theoretical framework. In what follows I will be working within a Relational Grammar (RG) framework, an outgrowth of generative theory which makes the methodological assumption that the grammatical relations 'subject of', 'direct object of', and 'indirect object of' are of primary importance to the syntactic systems of natural language.

Within the RG framework, then, we cannot remain neutral regarding questions about whether something is subject or object of a given verb, or even whether impersonal verbs have a subject or not. And within RG, there are proposed universal "laws" regarding the syntactic effects of derivational processes commonly found in languages investigated within some variant of generative grammar. These laws, along with independently supported facts about the language under investigation, can help investigators answer the questions mentioned at the beginning of this paragraph. (While these laws won't play an important role in this paper, I mention them here as part of my intent to encourage research on the topic of this paper.)

4.1 We saw in 1. that impersonal verbs are II in surface structure, and furthermore Blackfoot subject copy-raising indicates that these verbs do have an inanimate subject at the point where copy-raising takes place. So I suggest that an abstract dummy subject is added to these logically subjectless verbs, and subsequent copy-raising and agreement rules are sensitive to the presence of this inanimate singular dummy.

4.2 Normally AI verbs with unspecified subjects appear to be II on the basis of surface inflection in several of the languages, though apparently are not II in Blackfoot (see 2.). For those where such verbs are II, if we can find syntactic support for an inanimate subject then we would also want to say here that an abstract dummy subject has been inserted, and at the same time a derivational formative (e.g. -naɪniw in Mistassini) is added to the verb. If, on the other hand, no syntactic evidence is found that these verbs have an inanimate syntactic subject, then the derivational formative is added simply to mark lack of a specified subject and the subjectless verb is then inflected for the most unmarked category, inanimate singular.

4.3.1 As for normally TI verbs with unspecified subject, we saw in 3. that most of these appear to be II in surface structure. As we said at the end of section 3., this would be explained if we could find additional evidence that the logical (inanimate) object of these verbs has been advanced to become subject. In Mistassini , then, this analysis would account for the addition of the derivational suffix -kani to the TI verb as a marker that the underlying object has been advanced to subject.

As for Delaware's use of the suffix -a·si in the independent order, the fact that Goddard calls it a "derived passive" suggests that the underlying object has been advanced to subject; if this is the case, then it is likely that the underlying object of corresponding conjunct order verbs is also a surface subject, despite the difference in morphological treatment.

4.3.2 Finally, we come to the most controversial set of forms: TA verbs with unspecified subjects. I promised in the introduction to discuss whether or not these are "passives". Of course, if "passive" simply means "logical subject is unspecified", then all the forms we have discussed in this paper are passive; that is exactly how Bloomfield (1958:33) used the term in his description of Eastern Ojibwa.[4] On the other hand, if "passive" refers to a focussing or topicalization process which makes an object more prominent than a subject, then it would be more appropriately used in Algonquian to describe obviation of subjects with proximate objects (see e.g. Frantz 1966:51 or Hockett 1966:61).

But within RG, the term 'passive' generally has a technical meaning, defined as advancement of an object to become subject. So in the discussion that follows, this will be the main point at issue: what evidence is there that the logical object of TA verbs with unspecified subject has been advanced to subject status?

One consequence of passivization is that the resultant verb is intransitive, the underlying object having been advanced. And there is some evidence that the verbs under discussion here are intransitive on the surface. First of all, in all the languages, affixes may be segmented which are identical or very nearly identical to those of the regular AI paradigm. In particular, some of the languages have the reflex of Goddard's (1974:322) *Hm in their AI paradigm but not in their TA paradigm, and these same languages have the reflex of *Hm in the TA unspec. subj. paradigm. One piece of evidence in Eastern Ojibwa which can be used to argue that these forms are intransitive is the secondary derivation of an intransitive verb with unspecified subject. That is, the putative passive of a TA verb can subsequently be marked as having no specified subject in the same way that normally AI verbs are (see 2.). Bloomfield (1958:44) says that _-iwa:n_ marks unspecified subject of AI verbs, with the accompanying implication of plurality of actors. And then (p.47) he lists what he terms an "impersonal passive" form of a TA verb, which has _-iwa:n_ attached to the unspecified subject form of a TA verb: _ki:-no:ppenanuwa:n_ 'there was pursuing of animate objects'. Note that my analysis here requires that passivization first replaces the unspecified subject of the TA verb with the unspecified logical object. While this may sound a bit bizarre, observe that the passivization rule would have to be more complicated to prevent this happening. Also, the ordering application of these rules is that predicted either by the principle of maximal "feeding" or by allowing rules to apply whenever their conditions of application are satisfied; thus there would need to be no statement of extrinsic ordering of these two rules. The point of most relevance here, however, is that there would be no reason to expect the rule that adds *-iwain* to apply to a verb with unspecified *object*, which is what we would have to claim has happened if we deny that 'be pursued' is passive in Menomini.

Goddard (1969:120) is the only investigator I am aware of to present syntactic evidence that we cannot simply say that the logical object of an unspecified-subject TA verb remains an object. I will not use his Delaware examples here, but instead will use Blackfoot examples which are parallel to Goddard's. The argument will be based on the independently demonstrable fact that as in all other Algonquian languages, there are preverbs which must be understood as having the same logical subject as the verb to which they are attached.[5] Thus the logical subject of *iito-* 'go (to do_)' must be the same as that of the verb to which it is attached, as in (9):

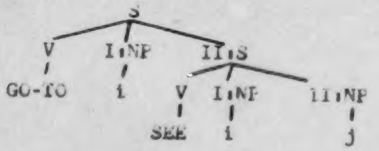
(9) nits-iito-oyi 'I went and ate'

And when attached to a transitive verb, the logical subject of *iito-* must be understood as coreferential with the subject of the transitive verb, not with the object:

(10) nits-iito-yinoawa { 'I went and saw him$_j$'
*'he$_j$ went and I saw him$_j$'

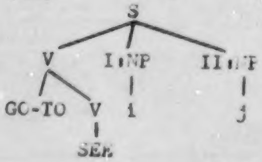
Treating preverbs like *iito-* as higher predicates which take a proposition as argument, as shown in (11), we can require that only when the 'upstairs' and 'downstairs' subjects are coreferential can we form a 'clause union' with a complex surface verb made up of the upstairs and downstairs verbs, as shown in (12), which would ultimately lead to (10).[6]

(11)



⇓ Equi-Subject Union

(12)

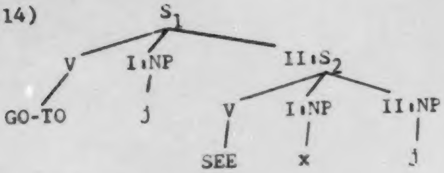


But when such complex verbs have the unspecified subject form, as in (13), we find that they are ambiguous:

(13) iito-yinoawa (a) 'he$_j$ was gone to be seen (i.e. unspecified subject went and saw him)'
(b) 'he$_j$ went and was seen'

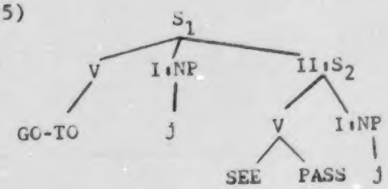
I.e., the subject of 'go' may be understood to be either the logical (unspecified) subject of 'see' or the logical object of 'see'. To explain how this verb can have the (b) meaning and maintain the constraint regarding equi-subject union, we must say that the downstairs verb has j as its subject when union takes place; i.e. that the downstairs clause has been passivized. We can diagram the relevant steps of the derivation as (14)-(16):

(14)


$S_1$
V
I:NP
II:$S_2$
GO-TO
j
V
I:NP
II:NP
SEE
x
j

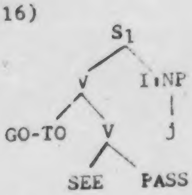
$\Downarrow$ II → I (Passivization)
(in $S_2$)

(15)


$S_1$
V
I:NP
II:$S_2$
GO-TO
j
V
I:NP
SEE
PASS
j

$\Downarrow$ Equi-Subject Union

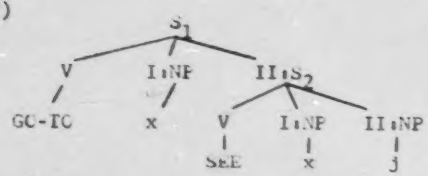
(16)


$S_1$
V
I:NP
GO-TO
V
j
SEE
PASS

So we see that the fact that (13) can be derived from (14) is support for a rule of passivization.[7]

Now, to explain how (13) can have the (a) meaning we must have a plausible derivation of (13) from (17); I propose that shown in (17) - (19):

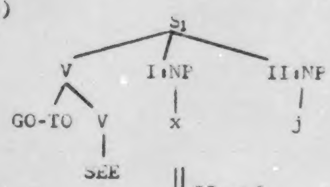
(17)



S1
V
I:NP
II:S2
GO-TO
x
V
I:NP
II:NP
SEE
x
j

⇓ Equi-Subject Union

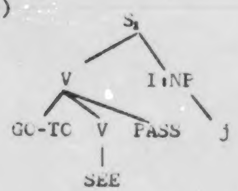
(18)



S1
V
I:NP
II:NP
GO-TO
V
x
j
SEE

⇓ II → I

(19)



S1
V
I:NP
GO-TO
V
PASS
j
SEE

The alert reader may ask what prevents passivization of $S_2$ before clause union, as we would expect if, as RG claims, rules which affect grammatical relations are always cyclic. The only answer I can suggest is that its application is blocked by a global constraint against any process which would ultimately prevent Equi-subject Union from applying to configurations containing equi-subject predicates like GO-TO.[8] This would seem less ad hoc if space permitted my including derivations with Micmac predicate WANT. The latter has two surface realizations, one when Equi-Subject Union has applied and the other when it has not applied. Since Equi-subject Union is not obligatory for this predicate, Micmac does allow passivization to apply downstairs in a configuration like (17) with WANT in place of GO-TO.

I am relatively certain that the same kind of ambiguity can be found in every Algonquian language as that seen in (13) above, having seen examples in both Delaware and Micmac. Thus the same kind of argument presented above for Blackfoot can be constructed for each of the languages.

In 3. we saw good inflectional evidence that Micmac transitives with unspecified subject are AI verbs. Now we will attempt to support this with syntactic evidence that the logical object has become subject.

Micmac has a rule which raises copies of subjects up to become objects of certain higher predicates. Unlike Blackfoot, in Micmac this rule applies only to subjects. Thus (21) is synonymous with (20), but (22) is bad because it would result from object copy-raising:

(20) pua:tɨm-Ø negɨm pma:l-nin
want(TI)-1 3 carry-2
'I want him to carry you'

(21) pua:l-k (negɨm) pma:l-nin
want(TA)-[1-3]

(22) *pua:l-ul negɨm pma:l-nin
want(TA)-[1-2]

Applying this test to a sentence like (23) with a putative passive in the complement, we find that (24) is good, supporting the claim that the logical object of the complement is a subject in (23):

(23) pua:tim-0 pma:l-uksi-n

want(TI)-1 carry-pass-3

'I want him to be carried'

(24) pua:l-k pma:l-uksi-n

want(TA)-[1-3]

In conclusion, I have shown there is evidence that transitive predicates with unspecified subjects are passives in Eastern Ojibwa, Blackfoot, and Micmac. This evidence is certainly not overwhelming, and it is my hope that other investigators will assist in the search for evidence to confirm or falsify the hypothesis that Algonquian unspecified subject forms for transitives are passive.

## NOTES

1 This research was supported in part by a National Museum of Canada contract, which help I gratefully acknowledge.

2 For readers not familiar with terminology and abbreviations which have become standard in Algonquian literature, four main verb types are distinguished: AI = intransitive verb with animate gender subject; II = intransitive verb with inanimate gender subject; TA = transitive verb with animate object; TI = transitive verb with inanimate subject. The term "conjunct" refers to an inflectional paradigm used for most subordinate clauses.

3 Where no source is indicated for language examples, they are from my own notes; in the case of Micmac, the data were collected with the help of Watson Williams, and Mistassini data were collected through the cooperation of Rod and Lieselotte Bartlett. In quoting published data, I have kept the original author's transcription, except that for ease of typing I have substituted the colon for a raised dot or macron. In Rogers' Mistassini data I have made the following substitutions: i for I; u for V; a for A; a: for a; i: for i; and u: for _.

4 Except that he didn't apply the term to the 'impersonal' verbs of 1.

5 Greg Thomson originally pointed out to me the value of this constraint as a test for subject-hood, in arguments he construct-

ed against the claim that obviative-on-proximate verb forms are passive.

6 Abbreviations in these diagrams include: V = predicate; S = proposition; NP = nominal argument; I = subject; II = direct object; i = referential index for speaker; j = arbitrary index for some third-person; x = unspecified index.

7 Contrary to my previous analysis (Frantz 1971:40, 41).

8 For readers who are familiar with the terminology, the 'like-subject' constraint on predicates like GO-TO makes reference to 'cyclic subjects.'

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# SOUTHERN NEW ENGLAND SHAMANISM: AN ETHNOGRAPHIC RECONSTRUCTION

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## INTRODUCTION

This paper is a portrayal of the shamanistic world view among the Algonquian people of southern New England in the period between the first Puritan arrivals in 1620 and the final Christian conversion of the New England Indian remnants during the Great Awakening in 1742. The groups included are from north to south, the Massachusett, Pokanoket (Wampanoag), Nauset, Narragansett, Eastern Niantic, Pequot, and Mohegan, all of whom were related through marriage, trade, language, and culture. In selecting evidence I have concentrated primarily upon the writings of seventeenth and eighteenth century Englishmen who lived and travelled in New England. I have consulted also seventeenth century and later accounts of Algonquian people around the periphery of the southern New England area, and accounts of cultures within the area which were written at a later date. First I will present my interpretation of the cultural and social context of shamanism and then describe what I consider to be the shaman's distinctive powers and functions.

## I

### Cautantowwit (Kytan)

New England was alive with deities, known as manitos. These included gods of woman, man, children, animal species, the sun, moon, fire, water, sea, snow, earth, directions, seasons, winds, houses, sky, corn, and colors (Eliot and Mayhew 1834:201-202) (Gookin 1970:20) (Thorowgood 1650:6) (Trumbull 1870:337-342) (Williams 1874:88; 1936:124-125). According to Narragansett mythology, Cautantowwit, the southwest deity, created man and woman of a stone but disliked his first creation and made them a second time of wood. They believed this second couple to be the ancestors of all mankind--until the arrival of the English when they concluded that, ". . . English-mans God made English Men, and the heavens and Earth there! yet their Gods made them and the Heaven, and Earth where they dwell" (Williams 1936:123). They communicated with Cautantowwit known among the Pakanoket and Massachusett as Kytan, by means of sacrifice, prayers, and praise, but he was not known in person by the living. Edward Winslow wrote that among the Pokanoket, "At first, they say, there was no sachim or king, but Kiehtan, who dwelleth above in the heavens . . . . Never man saw this Kiehtan; only old men tell of him . . ." (Winslow 1841:356).

## Hobbamock (Chepi)

All persons, whether sachems or commoners had access to most other spirits through dreams and visions, which they pondered and carefully heeded. Dreams in my classification occur while the person is asleep and visions occur while awake. I have been unable to determine whether this distinction reflects native categories. Visions were reported to be spontaneous and induced, and both kinds were experienced privately by individuals as well as collectively by more than one person at a time.

The principal deity who appeared was Hobbamock (Abbomacho), who was identical apparently with the spirit known as Chepi (Cheepie, Chepian). The latter name was related to words for death, the departed, and the cold northeast wind (Josselyn 1833:300) (Trumbull 1903:22-23). Into his ". . . deformed likeness they conceived themselves to be translated when they died; for the same word they have for Devil, they use also for a Dead Man . . ." (Eliot and Mayhew 1834:202). Hobbamock was associated with the color black and was seen often at night in the shapes of persons, animals, inanimate objects, and mythical creatures. His appearance was startling and terrifying (Eliot and Mayhew 1834:202) (Josselyn 1833:300) (Winthrop I, 1825:254) (Wood 1865:86). "He appears not to all," wrote Edward Winslow, but to ". . . the chiefest and most judicious amongst them; though all of them strive to attain to that hellish height of honor" (Winslow 1841:357).

Pniese

The pniese was a person who attained a particular vision of Hobbamock in an ordeal. This status is attested in the Massachusett and Pokanoket areas, where they subjected the strongest and most able male children to loss of sleep, fasting, and drinking mixtures which altered consciousness. These included, ". . . the juice of sentry and other bitter herbs, till they cast, which they must disgorge into the platter, and drink again and again, till . . . it will seem to be all blood" (Winslow 1841:360). Josselyn wrote of a ritual in which the candidates drank white hellebore which he likened to opium:

> The English in New England take white Hellebore, which operates as fairly with them, as with the Indians, who steeping of it in water sometime, give it to young lads gathered together a purpose to drink, if it come up they force them to drink again their vomit, (which they save in a Birchen-dish) till it stays with them, & he that gets the victory of it is made Captain of

the other lads for that year (Josselyn 1833:251).

The initiates drank until, ". . . by reason of faintness, they can scarce stand on their legs, and then must go forth into the cold. Also they beat their shins with sticks, and cause them to run through bushes, stumps and brambles, to make them hardy and acceptable to the devil, that in time he may appear unto them" (Winslow 1841:360). The vision sought in this ordeal was probably the Thunder Bird, a dream visitor known elsewhere in Algonquian culture (Hallowell 1967:161) and apparently among twentieth century Mohegan known to Speck:

> A certain kind of cry in the woods at night is made, it is said, by the devil's bird. The bird makes its cry in one place and then goes on to another for a while. The sounds are said to resemble those made by owls, but need not be confused with them. The same bird is thought to have something to do with thunder (Speck 1909:203).

Hobbamock, "maketh covenant with them to preserve them from death by wounds with arrows, knives, &c." (Winslow 1841:359). By virtue of having attained this vision, the pniese acquired a voice as his sachem's counsellor, collected tribute annually from the sachem's subjects, and participated with him in decisions about war. He was a model male warrior, ". . . known by . . . courage and boldness, by reason whereof one of them will chase almost an hundred men. . ." (Winslow 1841:359).

Powwow

The shamans, known in the southern New England dialects as powwows, differed from other humans because one or several forms of Hobbamock entered their bodies in a vision or a dream. John Eliot asked two Massachusetts how one became a powwow and was told, ". . . that if any one of the Indians fall into any strange dreame wherein Chepian appeares unto them as a serpent, then the next day they tell the other Indians of it . . ." whereupon the others ". . . dance and rejoyce for what they tell them about this Serpent, and so they become their Powwaws. . ." (Wilson 1834: 19-20). A Wampanoag powwow on Martha's Vineyard who became a Christian confessed to Thomas Mayhew Jr. that he first ". . . came to be a Pawwaw by Diabolical Dreams, wherein he saw the Devill in the likeness of four living Creatures . . ." and then gave Mayhew the most detailed surviving account of a southern New England shaman's repertoire. One of the creatures:

> was like a man which he saw in the Ayre, and this told him that he did know all things upon the Island, and what was to be done; and this he said had its residence over his whole body. Another was like a Crow, and did look out sharply to discover mischiefs coming towards him, and had its residence in his head. The third was like to a Pidgeon, and had its place in his breast, and was very cunning about any businesse. The fourth was like a Serpent, very subtile to doe mischief, and also to doe great cures, and these he said were meer Devills, and such as he has trusted to for safety, and did labour to

> raise up for the accomplishment of any thing in his diabolicall craft . . . (Whitfield 1834b:186).

Samson Occum wrote of the Montauk shamans:

> As for the Powaws, they say they get their art from dreams; and one has told me they get their art from the devil, but then partly by dreams or night visions, and partly by the devils immediate appearance to them by various shapes; sometimes in the shape of one creature, sometimes in another, sometimes by a voice, . . . (Occum 1809:109).

Because of their access to a range of spirits both within and outside of themselves the powwows advised the sachems in decisions, but the inspired role of shaman and the hereditary role of sachem did not overlap generally in one individual. The few persons who combined these roles were thought to be extremely powerful (C. Mather II, 1820:481) (Morton 1836:25). Tispaquin, the "Black Sachem" of Assawamset (now Middleboro) who fought on the side of Philip in King Philip's War, was said to be ". . . such a great Pauwau, that no bullet could enter him . . ." (Church 1829:144). Most prominent early New England sachems (Massasoit, Philip, Canonicus, Miantonomi, Canonchet, Ninigret, Uncas) were not shamans. With one recorded exception, all shamans were male (Wilson 1834:19). The powwows do not appear to have been full time specialists although some profited more than others according to their reputations for curing, sorcery, and weather control. They fought in war, owned land, and married (Harris 1902:57-58) (Leachford 1867:118) (Rowlandson

1913:153) (Wood 1865:91). Although it was thought to be a ". . . very impious matter for any man to derogate from the words of these Powahs" (Morton 1947:26) they were subject to the sachem's authority (Winslow 1841:307-308).

Hobbamock appeared ordinarily and was most conversant with three categories of people:

> One, I confess I neither know by name nor office directly; of these they have few, but esteem highly of them, and think that no weapon can kill them; another they call by the name of powah; and the third pniese (Winslow 1841:357)

No other writer mentioned the first of these which may have been the role-combined powwow-sachem. A number of functional distinctions are apparent within the powwow category. The Narragansett as described by Williams had, ". . . an exact forme of . . . Priest, and Prophet . . ." which evidently were known by the term powwow:

> Their Priests, performe and manage their Worship: Their wise men and old men of which number the Priests are also,) whom they call Taupowauog make solemne speeches and orations, or Lectures to them, concerning Religion, Peace, or Warre and all things (Williams 1936:128).

Although English writers reported native equivalents to their categories "prophet", Priest", "witch" and, "over-Seer . . . of Worship" (Trumbull 1903:309, 345) (Williams 1936:128), the above statements by Winslow and Williams suggest to me that these referred to different powers which the shaman might have possessed and to the distinct functions which he consequently performed. Although several shamans participated occasionally in tribal and divinatory rites, no evidence clearly points to the existence of shamanistic societies.

## The Soul

Most English writers noticed paralells in Indian cosmology to their own expectations of salvation and damnation (Josselyn 1833:294) (Morton 1947:35) (Williams 1936:130) (Winslow 1841:356) (Wood 1865:105). Eliot observed among the Massachusett ". . . some principles of a life after this life, and that good or evill, according to their demeanor in this life" (Winslow 1834:82). Roger Williams' observations on Narragansett soul beliefs provide an inadvertant basis for an alternative interpretation of Indian cosmology. One word for soul, Cowwewonck, was:

> Derived from Cowwene to sleep, because say they, it workes and operates when the body sleepes. Michachunck the soule, in a higher notion which is of affinity, with a word signifying a looking glasse, or cleere resemblance, so that it hath its name from a cleere sight or discerning, which indeed seemes very well to suit with the nature of it (Williams 1936:130).

Both the southwest, home of Cautantowwit, and the northeast, related to the terrestrial spirits Hobbamock or Chepi, were associated with disembodied souls. One of the two souls indicated by Williams could have returned to the creator while the other remained among the living and re-entered shamans. One departing soul seems to have appeared as a light against the sky:

> They have a remarkable observation of a flame that appears before the death of an Indian or English upon their Wigwams in the dead of the night: The first time that I did see it, I was call'd out by some of them about twelve of the clock, it being a very dark night, I perceived it plainly mounting into the Air over our Church . . . look on what side of a house it appears, from that Coast respectively you shall hear of a Coarse within two or three days (Josselyn 1833:300).

Frank Speck published a Penobscot tale about a woman who dreamed accurately of events which occurred some miles away. While she slept a ball of fire was seen to leave her mouth, during which time she was feared dead. When the flame returned to her mouth she awakened and recounted the dream (Speck 1919:288). Twentieth century Mohegan, Nauset, and Wampanoag referred to what they translated as "ghost", "spirit," or "devil", seen as light in foxfire as dji.bai or tci.pai (Speck 1928:263). Williams' dream soul, Cowwewonck, would seem to be the entity known after death as chepi, the shaman's helper.

## II

### Calendrical Tribal Ritual

The principal ritual form in southern New England was the Nickommo, translated by Williams as "A Feast or Dance." These occured regularly, (perhaps) at planting time in spring, when corn ripened in summer, and at mid-winter early in January, and involved the sachem's participation (Gookin 1970:19) (Morton 1947:20) (Pynchon 1947:20) (Williams 1874:20, 56; 1936:127, 180). The powwows presided as orderers of worship and authors of visions:

> These doe begin and order their service, and Invocation of their Gods, and all the people follow, and joyne interchangeably in a laborious bodily service, unto sweatings, especially of the Priest, who spends himselfe in strange Antick Gestures, and Actions even unto fainting (Williams 1936:127).

Daniel Denton, the Seventeenth century author of A Brief Description of New York, described harvest rituals on Long Island in which "their chief Priest or pawaw" collected shell money from those in attendance for their god whom he did not name:

> The priest takes the money, and putting it into some dishes sets them upon the top of their low flat-roofed houses, and falls to invocating their God to come and receive it, which with many a loud hallows and outcries, knocking the ground with sticks, and beating themselves, is performed by the priest, and seconded by the people.
>
> After they have thus a while wearied themselves, the priest by his Conjuration brings in a devil amongst them, in the shape sometimes of a fowl, sometimes of a beast, and sometimes of a man, at which the people being amazed, not daring

> to stir, he improves the opportunity, steps out, and makes sure of the money and then returns to lay the spirit, who in the mean time is sometimes gone . . . (Denton 1670:8).

The Narragansett hosted an elaborate ritual in which the participants offered, ". . . almost all the riches they have to their gods, as kettles, skins, hatchets, beads, knives, & c.", all of which the powwows cast ". . . into a great fire that they make in the midst of the house, and there consumed to ashes" (Winslow 1841:359). Pokanokets associated this ritual with tribal well-being:

> This the other Indians about us approve as good, and wish their sachims would appoint the like; and because the plague hath not reigned at Nanohigganset as at other places about them, they attribute to this custom there used (Winslow 1841: 359).

Winslow, our only source on the property burning ritual, indicated that it was addressed to Kytan and that the Narragansett held it "at certain known time," which he did not disclose (1841:359).

### Divination

The sources contain numerous references to the shaman's role as diviner. For many weeks after the Pilgrims landed at Plymouth the Wampanoag and English watched each other supiciously, until an Indian named Samoset came among the settlers and prepared the way in broken English" . . . for the coming of their great Sachem Massasoit" (Bradford 1952:80). According to Bradford's hearsay report, Massasoit's decision to make friendship was preceded by an assembly of powwows from all over the area:

> . . . before they came to the English to make friendship, they got all the Powachs of the country, for three days together in a horrid and devilish manner, to curse and execrate them with their conjurations, which assembly and service they held in a dark and dismal swamp (Bradford 1952:84).

Shamans divined the cause of illness and learned whether or not it was curable. Reverend Samuel Lee of Bristol, Rhode Island (then Plymouth Colony) indicated that such knowledge was obtained from creatures seen as a crow, hawk, or rattlesnake (Kittredge 1913:151). Edward Ward, a late seventeenth-century visitor to Boston, described a ritual of unspecified provenience in which the *powwow* divined in trance:

> Upon the breaking out of a War, or such extraordinary Occasions, as the old *Romans* consulted their *Oracles*, so do the *Indians* their *Pawaws*, which are a kind of Wizards: And at a General *Pawawing*, the Country a Hundred Miles round assemble themselves in a Body; and when they are thus met, they kindle a large Fire, round which the Pawaw walks, and beats himself upon his Breast, muttering out a strange sort of intricate Jargon, until he has Elivated himself into so great an Agony, that he falls down by the Fire in a Trance; during which time, the *Sagamores* [Sachems] ask him what they have a mind to know: After which, he is convey'd thro'

> the Fire, in the same posture that he lies, by a Power invisible, in the sight of the Spectators; then awakes, and Answers the several Questions ask'd by their _Kings_ or _Sachems_ (Ward 1905:67).

Thomas Cooper of Gay Head provided a partly fanciful but I think mainly genuine account of trance and soul journey to Benjamin Basset an Englishman of that Island. Cooper obtained his information from a Wampanoag ancestress, whom he identified as 'grandmother,' who was born before 1642:

> BEFORE the English came among the Indians, there were two disorders of which they most generally died, viz. the consumption and the yellow fever. The latter they could always _lay_ in the following manner . . . .After the rich had thus given away all their moveable property to the poor, they looked out the handsomest and most sprightly young man in the assembly, and put him into an entire new wigwam . . . . They then formed into two files at a small distance from each other. One standing in the space at each end, put fire to the bottom of the wigwam on all parts, and fell to singing and dancing. Presently the youth would leap out of the flames, and fall down to appearance dead. Him they committed to the care of five virgins, prepared for that purpose, to restore to life again. The term required for this would be uncertain, from six to forty-eight hours; during which time the dance must be kept up. When he was

> restored, he would tell, that he had been carried in a large thing high up in the air, where he came to a great company of white people, with whom he had interceded hard to have the distemper layed; and generally after much persuasion, would obtain a promise . . . which never failed of laying the distemper (Basset 1806:140).

Common elements recur in the rituals described by Ward and Cooper. A person (a shaman in Ward) enters trance, the trance state is associated with insensitivity to fire, the trancer communicates with otherwise inaccessible powers (celestial spirits in Cooper), and upon his return he reports the experience. In the ritual which Cooper described the trance state was thought of as a form of death. I attribute this belief to the ritually induced temporary absence of the dream soul, which left forever at death.

If Indians had written their history of King Philip's War, they would have emphasized, I think, the importance of shamanistic divination. On the morning of May 8, 1676, a party of more than three hundred Indians appeared in the rain-soaked meadows around the town of Bridgewater, Massachusetts. They burned several buildings, a thunder and lightning storm broke overhead, and they withdrew without killing any of the twenty-six male defenders. According to a legend told in the area long after King Philip's War had ended, the warriors ". . . had a Pawaw when the Devil appeared in the Shape of a Bear walking on his 2 hind feet; . . ." If the appearance had been a deer the Indians said, ". . . they would have destroyed the whole Town & all the English." Because the vision was that of a bear, they ". . . all followed him & drew off" (Stiles 1916:232). The leader of these warriors was Tispaquin, the warrior sachem of Assawamset who was a shaman (Hubbard I, 1865:190). The relation between the bear vision and retreat is clarified in a passage from Van der Donck's New Netherlands in which the appearance of a ravenous animal was identified as a bad omen:

> . . . they assemble in the afternoon towards evening, . . . They begin with jumping, crying, and grinning, as if they were possessed and mad. . . . When their charming has continued some time, then the devil, as they say, appears to them in the form of a beast. If the beast be a ravenous animal, it is a bad omen; if it be a harmless creature, the sign is better; the animal gives them strange answers to their inquiries, but seldom so clear and distinct that they can comprehend or interpret the same. . . . If there be any Christians present on those occasions, who observe all their doings, then the devil will not appear (Van der Donck 1841:203).

Trance mediumship may have been involved in a curious ritual described in the Narrative of the Captivity of Mrs. Mary Rowlandson. Mary Rowlandson, wife of the minister of Lancaster, Massachusetts, was captured early in 1676, when Philip's men fell upon that frontier settlement. In April of 1676, her captors and numerous other bands attacked and devastated an English force in the battle for Sudbury. Before that battle Rowlandson witnessed a ritual which she described in detail, but which she did not understand:

> Before they went to that fight, they got a company together to Powaw; the manner was as followeth. There was one that kneeled upon a Deerskin, with the company round him in a ring who kneeled, and striking upon the ground with their hands, and with sticks, and muttering and humming with their mouths; besides him who kneeled in the ring, there also stood one with a Gun in his hand: Then he on the Deer-skin made a speech, and all manifested assent to it: and so they did many times together. Then they bade him with the Gun go out of the ring, which he did, but when he was out, they called him in again; but he seemed to make a stand, then they called the more earnestly, till he returned again: Then they all sang. Then they gave him two Guns, in either hand one: And so he on the Deerskin began again; and at the end of every sentence in his speaking, they all assented, humming or muttering with their mouthes, and striking upon the ground with their hands. Then they bade him with the two Guns go out of the ring again; which he did, a little way. They they called him in again, but he made a stand; so they called him with greater earnestness; but he stood reeling and wavering as if he knew not whither he should stand or fall, or which way to go. Then they called him with exceeding great vehemency, all of them, one and another: after a little while he turned in, staggering as he went, with his Armes stretched out, in either hand a Gun. As soon as he came in, they all sang and rejoyced

> exceedingly a while. And then he upon the Deer-skin, made another speech unto which they all assented in a rejoicing manner: and so they ended their business, and forthwith went to Sudbury-fight (Rowlandson 1913:152-153).

The standing warrior with the muskets behaved as if he had been entranced by the powwow and other participants and then recalled. The shaman felt assured concerning the attack for, ". . . they went without any scruple, but that they should prosper . . . . they acted as if the Devil had told them that they should gain the victory . . ." (Rowlandson 1913:153).

Divination influenced morale. Rowlandson noted that when the warriors returned from the Sudbury fight, which they won, they were depressed, as if they had learned of their doom. Cotton Mather reflected on the cause of this transformation:

> But NOW was the time for deliverance! There was an evil spirit of dissention strangely sent among the indians, which disposed them to separate from one another: the daemons, who visible exhibited themselves among them at their powawing or conjuring, signified still unto them, that they could now do no more for them . . . an unaccountable terror . . . so dispirited them, that they were like men under a fascination (C. Mather II, 1820:495).

To summarize, the powwow's divination techniques included casting visions which he read as good or bad omens, direct encounters between his soul and other spirits, and insight available through the ability of his helping spirit(s) to fly and view events in the past, future, and at a distance. In addition to providing answers to questions about the cause of illness, the outcome of future actions, and choices in forthcoming decisions, the powwow as diviner identified thieves (M. Mayhew 1694:12) and murderers (Hopkins 1911:35-36).

Omnipotence

Tequanonim, a notorious powwow of Martha's Vineyard converted to Christianity and disclosed the identity of his guardian spirits:

> . . . he as they said, and in their ignorance conceived, never did hurt to any, but always good . . . . And as himself said he had been possessed from the crowne of the head to the soal of the foot with Pawwawnomas, not onely in the shape of living Creatures, as Fowls, Fishes, and Creeping Things, but Brass, Iron, and Stone (Whitfield 1834:186-187).

Whether their power originated in fishes or other aquatic animals (Speck 1919:281) certain powwows believed themselves to be capable of swimming under water. Roger Williams heard of one such claim by a Pequot shaman which he described in a letter to John Winthrop:

> The Pequots hear of your preparations, &c., and comfort themselves in this, that a witch amongst them will sink the pinnaces [English vessels], by diving under water and making holes, &c., as also that they shall now enrich themselves with store of guns, but I hope their dreams

> (through the mercy of the Lord) shall vanish, and the devil and his lying sorcerers shall be confounded (Williams 1874:6).

Some shamans claimed the ability to resist arrow, hatchet, and musket shot wounds. English soldiers who fought at the Pequot stronghold in Mystic in 1637 apparently knew of this belief:

> There were some of these Indians, as is reported, whose bodyes were not to be pierced by their sharp rapiers or swords of [for] a long time, which made some of the Souldiers think the Devil was in them, for there were some Powwowes among them, which work strange things with the help of Satan (Johnson 1910:168).

Tispaquin, the leader of the party that saw the bear vision at Bridgewater was said to be impenetrable by musket balls and had been struck twice with no effect. He surrendered near the close of King Philip's War expecting to serve as a Captain under Benjamin Church, but, was put before a firing squad at Plymouth where he died.

### Weather

A few details survive of rain making rituals. "If the yeere proove drie" wrote Williams, "They have great and solemne meetings . . . at one high place, to supplicate their gods; and to beg raine . . ." (Williams 1936:67). Uncas once summonded his shamans and a reputable shaman from another tribe to end a drought at Mohegan, which they failed to do. Exasperated, he asked Reverend James Fitch of Norwich to pray in his behalf. Uncas wanted rain, and not religion (Gookin 1970:83, 109-110) (Whitfield 1834a:140) and in this incident he used English ritual performed by an English minister as he used political alliance with the United Colonies, as an instrumont for his enhancement and survival. Although Penobscot shamans fought magical duels across social boundaries, (Speck 1919:243), I know of no evidence that such contests occured in southern New England. Fitch accepted Uncas' request with a competitive spirit:

> . . . then Uncas with many Indians came to my House, Uncas lamented there was such Want of Rain; I asked, whether if God should send us Rain, he would not attribute it to their Pawawes? He answered, No, for they had done their Uttermost, and all in Vain: I replyed, if you will declare it before all these Indians you shall see what God will do for us; . . . Then Uncas made a great Speech to the Indians (which were many) confessing that if God should then send Rain, it could not be ascribed to their Pawawing, but must be acknowledged to be an Answer of our Prayers. This Day the Clouds spread more and more: and the next Day there was such a Plenty of Rain, that our River rose more than two foot in Height (Fitch, in Hubbard I, 1865: 289-290).

Some persons, ". . . can cause the wind to blow in what part they list -- can raise storms and tempests, which they usually do when they intend the death or destruction of other people . . ." (Winslow 1841: 366). Perhaps the greatest weather feat for which the powwows took credit was a hurricane that battered the Boston area in August of 1675. The Indians who for three months had been at war with the English, " . . . reported that they had caused it by their Pawwaw . . . They farther say, that as many Englishmen shall die, as the Trees by this Wind have been blown down in the Woods . . ." ([Saltonstall] 1867: 158). Since the arrival of the English and their God in Massachusetts Bay, wrote William Wood, the Devil's tenure in that place had slipped. One proof was an improvement in weather:

> They [the Indians] acknowledge the power of the Englishmans God, as they call him, because . . . the times and seasons being much altered in seven or eight yeares, freer from lightning and thunder, long droughts, suddaine and tempestuous dashes of raine, and lamentable cold Winters (Wood 1865:94).

Hunting

Hunting ritual existed but descriptions are lacking. In 1677 warriors of unspecified origin raided Deerfield, Massachusetts and captured an Englishman named Quentin Stockwell who mentioned shamanistic efforts to improve hunting in his captivity narrative:

> . . . all the Indians went a hunting, but could get nothing: divers days they powow'd, but got nothing; then they desired the English to pray, and confessed they could do nothing; they would have us pray, and see what the Englishman's God could do. I prayed, so did Serjeant Plimpton, in another place. The Indians reverently attended morning and night;

> next day they got bears; then they would needs have us desire a blessing, return thanks at meals: after a while they grew weary of it, and the Sachim did forbid us (I. Mather 1890: 33-34).

In this case as in the one involving Uncas and Reverend Fitch, Indians used Christian ritual performed by believers as a tool in their behalf.

Curing

The powwow as curer first divined to learn if an affliction was curable:

> Another power they worship, whom they call Hobbamock . . . is the devil. Him they call upon to cure their wounds and diseases . . . . but when they are mortal and not curable in nature, then he persuades them Kiehtan is angry, and sends them, whom none can cure . . . (Winslow 1841:356-357).

One or more shamans, the patient, an audience, and a variety of spirits participated in the cure, which sometimes took several hours. Winslow and Hobbamock found the ailing Massasoit in the midst of one such gathering when they hiked from Plymouth to his home at Sowams, now Warren, Rhode Island:

> When we came thither, we found the house so full of men, as we could scarce get in, though they used their best diligence to make way for us. There were they in the midst

> of their charms for him, making such a hellish noise, as it distempered us that were well, and therefore unlike to ease him that was sick. About him were six or eight women, who chafed his arms, legs, and thighs, to keep heat in him. When they had made an end of their charming, one told him that his friends, the English, were come to see him (Winslow 1841: 317-318).

The powwow and audience commenced with a musical invocation and throughout the ritual he sang and the onlookers responded and sometimes joined with him, "like a quire" (Williams 1936:198). His utterances are described as "horrible outcries" and "hollow bleatings" (Eliot and Mayhew 1834:204) some of which resembled animal cries. His physical movements involved "odd," "fierce," and "laborious" gestures over the patient which resulted in profuse sweating, "until they foam" (Gookin 1970:20). The most extraordinary feature of the cure, noted also in divination ritual, was the shaman's self-beating of his chest and thighs:

> The parties that are sick or lame being brought before them, the Pow-wow setting downe, the rest of the Indians giving attentive audience to his imprecations and invocations, and after the violent expression of many a hideous bellowing and groaning, he makes a stop, and then all the auditors with one voice utter a short Canto; which done, the Pow-wow still proceeds in his innovations, sometimes roaring like a Beare, other times groaning like a dying horse, foaming at the mouth like a chased bore, smiting his

> naked brest and thighs with such violence, as if he were madde. Thus will hee continue sometimes halfe a day, spending his lungs, sweating out his fat, and tormenting his body in this diabolical worship; . . . (Wood 1865: 93).

Williams added that the powwow, "threaten[s] and conjures out the sicknesse" (1936:127). Such actions resulted in "extasie" (Lechford 1867:118). He removed alien objects from the patient's body by the invisible cooperation of his internal spirits which was seen by others as laying on hands, sucking, and spitting or blowing the objects away (Kittredge 1913:152) (Wilson 1834:20) (Winslow 1841:357). If a wound proved to be curable, Winslow was told, " . . . he toucheth it not, but askooke, that is, the snake, or wobsacuck, that is, the eagle, setteth on his shoulder, and licks the same. This none see but the powah, who tells them he doth it himself" (Winslow 1841:357-358).

A powwow accused of causing illness could be forced to cure it himself. A Martha's Vineyard resident found relief in this way:

> . . . his Friends advised him to the Powaw's, concluding him to be Bewitched; they being met, and dancing round a great Fire, the Sick lying by; some of the Neighbours entered the House, being perswaded that a great Powaw, now called to cure, had bewitched the Sick: they threaten him that as he had Bewitched, unless he would Cure the Sick man, they would burn him in that fire; after many Excuses, too long here to relate, they took him up, resolving at least to a little

> Singe him; who no sooner felt the heat of the fire near him, but the Sick immediately recovered; . . . (M. Mayhew 1694: 14).

To the assurance that he too could see Hobbamock during a cure, Winslow replied to, ". . . the contrary, which so proved; yea, themselves have confessed they never saw him when any of us were present" (Winslow 1841:358). A successful powwow showed his helper to the audience:

> The Pawwaws counted their Imps their Preservers, had them treasured up in their bodies, which they brought forth to hurt their enemies, and heal their friends; who when they had done some notable Cure, would shew the Imp in the palm of his Hand to the Indians who with much amazement looking at it, Deified them . . . (Eliot and Mayhew 1834: 202).

If the patient survived the admirers cried, "Much winnit Abbamocho, that is, very good Divell . . ." (Johnson 1910:263). The Pequot women gave this praise to the English soldiers who devasted their village at Mystic in 1637 (ibid:168).

A number of writers acknowledged that powwows cured patients. Williams criticized them because they, ". . . doe bewitch the people, and take their Money . . ." and discredited their techniques, yet affirmed that they ". . . doe most certainly (by the help of the Divell) worke great Cures . . ." (1936:198). Their methods succeeded with what might be described as psychological as well as physical disorders and included practical skills such as bone setting and wound binding (Gookin 1970:20) (Lechford 1867:117).

## Sorcery

Although the powwow was not responsible for all the mis-deeds which Indians and Englishmen attributed to him, some did bewitch others deliberately. He directed his sorcery against personal enemies and political rivals in his own behalf and in behalf of paying clients both within and outside of the sachemdom.

Magical intrusion and dream soul capture best describe his two techniques. In the former case, he prepared a physical object such as a leather arrowhead, a hair, or a bone from some dead creature, which his ally conveyed to the victim's body. The spirit was reported to become, ". . . the real body of a Serpent, which comes directly towards the man in the house or in the field, looming or having a shadow about him like a man, and do shoot a bone (as they say) into the Indian's Body, which sometimes killeth him" (Eliot and Mayhew 1834:204). One woman who, "lay in great Extremity and wholly impotent . . ." could not be cured by local shamans and her relatives, ". . . sent to Martha's Vineyard, for more famoused Powaws . . .", one of whom caught the alien spirit in a deerskin and, "she immediately recovered . . . ." This spirit they determined was, ". . . the Spirit of an Englishman drowned in the Adjacent Sound . . ." which was in the control of a powwow who lived in the woman's vicinity. The powwow who caught the spirit told her that, ". . . unless she removed to Martha's Vineyard, she would again be Sick, for being an English Spirit he could not long confine it" (M. Mayhew 1694:15). Clearly the relationship between the powwow and this spirit helper was not geneological or ancestral. The unruliness of the spirit suggests an unwitting mid-seventeenth century appraisal of eroding Indian autonomy.

The powwow responsible for the only explicit recorded case of sorcery between close kin claimed to have made a mistake:

> Another well known powaw designing to kill an Indian, who accidentally lodg'd in the house with him and his brother, went forth to inchant an hair. While he was abroad, his brother alter'd his place about the fire, where they slept, and the strange Indian came into his place. The conjurer coming in with his devilish implement, gave it a direction to the back of his enemy, which by his mistake, proved his brother, and the devil therewith immediately kill'd him (C. Mather II, 1820:387-388).

Powwows acknowledged and others observed that magical objects and intrusive spirits entered the victim's body, ". . . without any outward breach of the Skin . . ." (M. Mayhew 1694:13). Samson Occum wrote of, "poisoning one another" by Montauk shamans, which, ". . . they say is no imaginary thing, but real. I have heard some say, that have been poisoned, it puts them into a great pain, and when a powaw takes out the poison they have found immediate relief . . ." (Occum 1809:109). Occum's "poisoning" seems to have been a form of magical intrusion.

Dream soul capture was the second important technique:

> . . . by their Seizing something of the Spirit (as the Devil made them think) of such they intended to Torment or Kill, while it wandered, in their sleep: this they kept being in the Form of a Fly, closely imprisoned; and according as they dealt with this, so it Fared with the Body it belonged to (M. Mayhew 1694:14).

Occum may have referred to soul capture sorcery when he wrote that, "at other times" Montauk victims:

> . . . feel no manner of pain, but feel strangely by degrees, till they are senseless, and then they will run mad. Sometimes they would run into the water; sometimes into the fire; and at other times run up to the top of high trees and tumble down headlong to the ground, yet receive no hurt by all these (Occum 1809:109).

New England shamans lacked confidence in their ability to injure English by sorcery. An Indian, probably a Pequot, who lived in Stonington, Connecticut, hired a Long Island powwow to bewitch an Indian neighbor with whom he quarrelled. Mr. Stanton, an English settler, confronted the powwow and validated whatever doubts he or his peers may have had:

> . . . sometime after the English lived at Stonington, there came an Indian (of that place) to Mr. Stanton (who had the

> Indian tongue) and told Mr. Stanton, there was an Indian (of that place) that had a quarril with him, and had sent for a greate powaw from Long Island, who had undertaken to revenge the quarril; and thereupon shewed a greate feare; whereupon Mr. Stanton sent for the powaw, and desired him to desist, telling him that Indian was his pertecaler friende, but the powaw refused without so greate a rewarde might be giuen, that the Indian could not be able to giue, and the Indian powaw grew still more high and positive in his language, until he told Mr. Stanton he could immediately tare his house in pieces, and himself flye out at the top of the chimney; and grew at length to be so daring that he raised the old gentlemans Temper, so that he started out of his great chayre and layed hold of the powaw, and by main strength took him, and with a halter tyed his hands, and raised him up to a hook in the Joyse, and whipped him untill he promised to desist and go home, which he did and the poore fearefull Indian had no harm from the powaw; there were many Indians witnout the house, who came as neare as they dare, and saw the disipline, and expected the house to be tore in pieces (as they said), who, when they saw the matter so concluded went away much Surprised (The Indian Powow 1848:44).

The small sample of available data indicates that persons sent or expected sorcery from others with whom they had experienced overt conflict and that some curable illness is traceable through the cultural explanation of sorcery to the covert actions of human agents who were linked in interpersonal conflict (Hazard ii, 1794:359) (The Indian Powow 1848:44) (Williams 1945:7).

## Passaconnaway

Passaconnaway, powwow-sachem of the Pawtucket on the Merrimack River at the northern edge of Puritan settlement, was a celebrity among New England shamans. The missionary Eliot characterized him as, ". . . a great Witch in all mens esteem . . . and a very politick wise man" (Winslow 1834:82). Rumors of his feats reached Massachusetts Bay:

> . . . if wee may beleeve the Indians, who report of one Pissacannawa, that hee can make the water burne, the rocks move, the trees dance, metamorphize himselfe, into a flaming man . . . . in Winter, when there is no greene leaves to be got, he will burne an old one to ashes, and putting those into the water, produce a new greene leafe . . . and make of a dead snakes skinne a living snake . . . (Wood 1865:92-93).

Thomas Morton remarked on the "admirable perfection" of the Indian senses, ". . . the Salvages have the sence of seeing so farre beyond any of our Nation, that one would allmost beleeve they had intelligence of the Devill . . ." (Morton 1947:33). Others described them as, ". . . having quicke wits, understanding apprehensions, strong memories, with nimble inventions, and a quicke hand . . ." (Wood 1865:88), and "by nature admirably ingenious." I think that the audiences before which the powwow performed watched and listened closely, and that within his culture, he possessed superior perceptual and motor skills. Passaconnaway's fame and powers surpassed all others':

> . . . that Sachem or Sagamore is a Powah of greate estimation amongst all kinde of Salvages . . . [he] hath advaunced his honor in his feats or jugling tricks . . . to the admiration of the spectators whome hee endevoured to perswade, that he would goe under water to the further side of a river to broade for any man to undertake with a breath, which thing hee performed by swimming over & deluding the company with casting a mist before their eies that see him enter in and come out, but no part of the way hee has bin seene, likewise by out English in the heat of all summer to make Ice appeare in a bowle of faire water, first having the water set before him hee hath begunne his incantation according to their usuall accustome and before the same has bin ended a thick Clowde has darkned the aire and on a sodane a thunder clap hath bin heard that has amazed the natives, in an instant hee hath shewed a firme peece of Ice to flote in the middest of the bowle in the presence of the vulgar people, which doubtles was done by the agility of Satan his consort (Morton 1947:25-26).

When John Eliot first came to talk with Passaconnaway about Christianity, he ran off into the woods. Eliot returned in 1648 and this time the shaman listened to the missionary's words and replied:

> . . . he had never prayed unto God as yet, for he had never heard of God before, as now he doth. And he said further, that he did beleeve what I taught them to be true. And for his owne part, he was purposed in his heart from thenceforth to pray unto God . . .(Winslow 1834:82).

In 1660 Passaconnaway spoke at a "great Dance" on the Merrimack to say farewell to his children and subjects, and to warn them as a powwow and sachem who accepted the Christian God, to avoid war with the English should it come. A "person of Quality" communicated and English version of his message to William Hubbard, who included it in his A Narrative of the Troubles With the Indians:

> I am now going the Way of all Flesh, or ready to die, and not likely to see you ever met together any more: I will now leave this Word of Counsel with you, that you take heed how you quarrell with the English for though you may do them much mischief, yet assuredly you will all be destroyed, and rooted off the Earth if you do: . . . I was as much an Enemy to the English at their first coming into these Parts, as any one whatsoever, and did try all Ways and Means possible to have destroyed them, at least to have prevented them sitting down here, but I could in no way effect it; (it is to be noted that this Paffaconaway was the most noted Pawaw and Sorcerer of all the Country) therefore I advise you never to contend

> with the English, nor make War with them (Hubbard I, 1865:48-49).

Accordingly, when war came in 1675, his eldest son, ". . . withdrew himself into some remote Place . . ." (Hubbard I, 1865:49).

Wetucks

Williams heard, ". . . many strange Relations of one Wetucks, a man that wrought great Miracles amongst them, and walking upon the waters, &c. with some kind of broken resemblance to the Sonne of God" (1936: to the Reader).

The Indian inhabitants of Cape Cod and Martha's Vineyard told stories of a giant named Moshup as recently as the third decade of the twentieth century (Knight 1925: 134-137) (Tantaquidgeon 1930: 20-25). Wetucks and Moshup, according to Ezra Stiles, were names for the same creature (1916: 157). Thomas Cooper, of Gay Head narrated the earliest recorded Moshup tale:

> The first Indian who came to the Vineyard, was brought thither with his dog on a cake of ice. When he came to Gay Head, he found a very large man, whose name was Moshup. He had a wife and five children, four sons and one daughter; and lived in the Den. He used to catch whales, and pluck up trees, and make a fire, and roast them. The coals of the trees, and the bones of the whales, are now to be seen. After he was tired of staying here, he told his children to go and play ball on a beach that joined Noman's Land to Gay Head. He then made a

> mark with his toe across the beach at each end, and so deep, that the water followed, and cut away the beach; so that his children were in fear of drowning. They took their sister up, and held her out or the water. He told them to act as if they were going to kill whales; and they were all turned into killers, (a fish so called.). The sister was dressed in large stripes. He gave them a strict charge always to be kind to her. His mourned the loss of her children so exceedingly, that he threw her away. She fell upon Seconet, near the rocks, where she lived some time, exacting contribution of all who passed by water. After a while she was changed into stone. The entire shape remained for many years. But after the English came, some of them broke off the arms, head, &c. but the most of the body remains to this day. Moshup went away nobody knows whither. He had no conversation with the Indinas, but was kind to them, by sending whales, &c. ashore to them to eat. But after they grew thick around him he left them (Basset 1806: 139-140).

Historic shamans with their many remarkable feats and ability to transform substances from one into antoher continued on a modest scale what Wetucks was known to have done in mythical time.

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# CODING OF ROLE INFORMATION IN OJIBWA

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My concern here is with the decoding of basic role information -- "who does what to whom" -- from Ojibwa sentences, as shown by some examples from the dialect spoken at Parry Island, Ontario. We can begin by noting that in simple English sentences like "John hit Bill", word order provides a crucial signal to a hearer, allowing the actor and the victim to be identified. In Ojibwa, the sequencing of nominal and verbal constituents is normally quite irrelevant for conveying this kind of information. Rather, the signalling of role information depends on a matching or congruence among inflectional "tags" appearing in nouns and verbs, a phenomenon known in linguistics as cross-reference. It is the purpose of this paper to demonstrate how cross-reference works in Ojibwa, so that even a non-speaker can deduce the meanings of typical sentences provided the relevant constituents and their meanings are known. If the demonstration is successful, it will provide one more bit of evidence for the essentially logical character of the workings of Algonkian.[1]

My analysis is based on those of Bloomfield and Hockett, but it departs from tradition in a few ways which up to now have made this scheme easier to explain to unsophisticated students than to my Algonkianist colleagues. I ask them now to be patient, and to be prepared to look at some familiar data from a somewhat new point of view. I need what one linguist has called "a temporary suspension of disbelief".

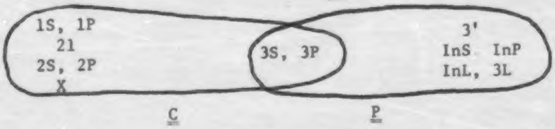
The crucial information as to "who does what to whom" evidently involves two distinct kinds of meanings. On the one hand, there are the *participants* involved in an event or situation under discussion. And on the other, there are the *roles* which these participants play. If we want to, we can conceptualize and talk about participants and roles. But in Ojibwa discourse these are always explicitly expressed: they are built into the language itself. I want to suggest that while participants and roles are potential subjects of discourse in English, in Ojibwa they are part of the means of discourse.

The Ojibwa participants and roles, which are signalled by the inflectional endings required for nouns and verbs, constitute two small sets. For purposes of discussion in Ojibwa, an entity must not only be codable as a particular noun: it must also be classified for a particular stretch of text as one from a limited number of participants given by the language. And the many possible relations among entities and events are similarly subjected to a language specific classification, involving a limited number of roles. These linguistic participants and roles are units of meaning which can be explicitly decoded from simple texts. They provide a crude but adequate base on which a linguistically competent and well enculturated Ojibwa presumably constructs a full and appropriate interpretation of a given text. My concern, then, is with the decoding rather than the interpretation of text, with literal meanings rather than with semantic subtleties.[2]

The various participants explicitly signalled in Parry Island Ojibwa are shown in (1). Each participant can be defined as a bundle of categories of the sort traditionally recognized in Algonkian. For example, the working symbol InS is an abbreviation for the cluster "third person, inanimate gender, singular number", translatable by English "it". The symbol 3' is an abbreviation for "third person, animate gender, obviative", normally glossed as "the other or others". 3L and InL are animate and inanimate locative participants, signalled only in nouns. Although locatives have not traditionally been treated as participants, comparable to proximates and obviatives, they evidently can appropriately be treated this way. In this dialect, number (S or P), obviative and locative are mutually exclusive categories: in every occurrence, each noun must be marked for exactly one of them. In terms of participants, we can suggest as a loose analogy that in a scene involving third persons, a proximate participant (marked as S or P) will likely be the star, obviatives will be the supporting players, while the locatives stand about like stage props.

As shown in the diagram (1),

(1) The Participants.



(3S and 3P -- proximates -- are C if the other participant in a transitive form is P, but P if this other is C.)
TWO P PARTICIPANTS CANNOT BE SIGNALLED IN THE SAME INFLECTED FORM.

the participants fall into two overlapping classes, labelled C and P for "central" and "peripheral", with third person singular and plural animate participants, the proximates, occupying the intersection. These two may be classed as either central or peripheral in a given case, central if the other participant represented in a transitive form is peripheral, but peripheral if this other is central. The two class labels -- any others would do equally well -- are needed for decoding, as will become clear below. And it is important to note that in this dialect there are no inflected forms representing two peripheral participants acting on each other. Transitive verbs can represent two central participants, provided they are different in person (I see you, you see me ...), or one central and one peripheral (I see him, he sees the other, the other sees him ...). But two peripherals cannot be represented in the same verb form. The same is true for possessed nouns. A possessed noun is always marked as a peripheral participant, with a central possessor. In a form translatable as "his dog", the possessor will be 3S, a central participant in this case, and the possessed "dog" will be obviative 3', peripheral. (In this approach, the "further obviatives" recognized by Bloomfield in Eastern Ojibwa have been reanalyzed, as will be taken up below.)

Item (2)

(2) *The Roles*.

| | | | |
|---|---|---|---|
| Signalled only in verbs : | (actor) | ACT | |
| | (patient) | PAT | GOAL |
| | (beneficiary) | BEN | |
| Signalled only in nouns : | (possessor) | POS | |
| | (possessed) | POSD | |
| | (orientor) | ORI | |
| Signalled in verbs and nouns : | (concerned) | CONC | |

list the different roles. Only the last two, *orientor* and *concerned*, require discussion, since these have not previously figured in descriptions of Algonkian.

The *orientor* role is a "catch-all", assignable to any nominal which does not already figure as an actor or a goal or a possessor. This will include all locatives, but sometimes other nouns and pronouns as well. Orientor nominals help to orient a verbal event or situation -- hence the name chosen for this role -- but location need not be involved. For example, in a sentence glossed as "Peter looks like John" (*mi· go e·žəna·guzid aw pi·tər ja·ning*), John can be marked as locative (*ja·ning*), signalling the orientor role, or as obviative (niw ja·nan 'that other one, John'). In this latter case John is still orientor, but by default now, since no other role is available for him.[3]

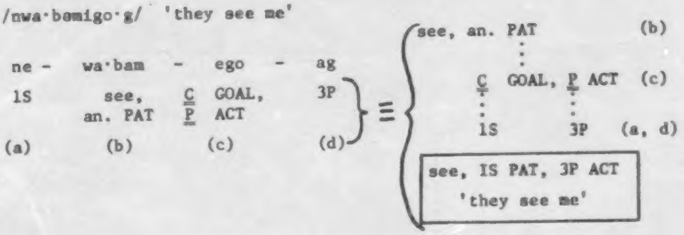
The other nontraditional role, concerned, is postulated to cover cases which Bloomfield handled in Eastern Ojibwa as further obviatives. In the current scheme, the concerned role is viewed as providing a special kind of escape hatch, used in sentences where the proximate/obviative contrast, as it is employed in this dialect, would lead to incorrect identifications among several animate third persons. In such cases, the concerned role can be allocated to an actor or to a possessor, signalling that this actor-concerned or possesor-concerned is "somebody else", not to be identified with some other third person actor in the context. The examples given in (7) below are cases of this kind and, incidentally, they are cases where the traditional analysis cannot account for the actual meanings expressed in these sentences.

Let us turn now to the examples.

Item (3)

(3) Role allocation within a transitive verb.

/nwa·bamigo·g/ 'they see me'



shows how role information can be deduced from a single transitive form, on the basis of the meanings of its constituents. The crucial element here is

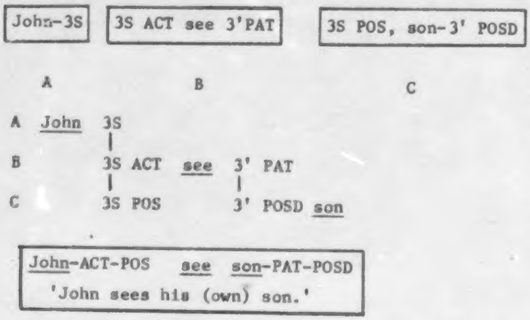
{-ego-}, the theme sign. This element identifies the major class of the actor and the goal, and the other affixes identify specific participants, one from each class. Substitution of a different theme {-a·-} 'P (an.) GOAL, C ACT' in this form results (predictably) in the meaning "I see them". The sequencing of elements within a transitive form is rigidly fixed and, accordingly, can have no significance for role allocation. (As linguists will note, traditional Algonkian "direct" and "inverse" forms can be anticipated as logical within this scheme.)

Item (4)

(4) Cross-reference: matching of verbal and nominal participants.

/ja·n uwa·boma·n ugwisan/ 'John sees his (own) son.'

ja·n-0 we-wa·bam-a·-an we-gwis-0-an


John-3S
3S ACT see 3'PAT
3S POS, son-3' POSD
A
B
C
A John 3S
B 3S ACT see 3' PAT
C 3S POS 3' POSD son
John-ACT-POS see son-PAT-POSD
'John sees his (own) son.'

Participants as a "means" of discourse: they allow roles to be linked with the lexical content of nouns.

shows how the role information provided by the individual constituents of a sentence can be related to the meaning of the sentence as a whole. The decoding of the constituents (A, B, C) is not shown here, but can be carried out in the way shown above for (3).[4] The "problem" for deducing the meaning of a sentence is that the actor and goal roles are explicitly signalled only in verbs, but not in the nouns. Cross-reference provides the typical Algonkian solution, by supplying matching links or tags, so that the roles signalled in verbs can be associated with the appropriately tagged nouns. When cross-reference is perfect, as in this example, the sequencing of the constituents can be varied, with no effect on the basic role information given in the sentence.

While decoding the role information in sentences like that of (4) is a transparently simple operation made possible by cross-reference, there are some predictable situations where cross-reference will not be applicable. Remember that it is not possible in this dialect to specify two peripheral participants within a single inflected form. If the actor and goal nouns in a sentence are both marked as peripheral participants, some kind of adjustment will have to be made, since a verb cannot show two peripheral participants as actor and goal.

If one of the nouns is obviative and other inanimate, as in (5),

(5) <u>Matching incomplete</u>.

With one obviative and one inanimate, an obviative noun will be identified with a proximate (ACT or GOAL) in the verb.

/uwa·bənda·n ji·ma·n ugwisan/ 'His son sees a boat.'

```
 we-wa·bəndam-n-∅         ji·ma·n-∅     we-gwis-an
┌────────────────────┐  ┌──────────┐  ┌──────────────────────┐
│3S ACT see InS PAT  │  │boat-InS  │  │3S POS, son-3' POSD   │
└────────────────────┘  └──────────┘  └──────────────────────┘
         A                    B                  C

     C  3S POS   son-3' POSD

     A                 3S ACT  see InS PAT
                                    |
     B                              InS boat
```

3S POS son-POSD-ACT see boat-PAT
'His son sees a boat.'

the inanimate noun will match an inanimate participant in the verb. But the other verbal participant will be represented as proximate (3S or 3P). The actor or goal role will be allocated to the inanimate noun through the normal matching, and the obviative noun will be assigned the other role by default. As in sentences where cross-reference is complete, the sequencing of constituents can be varied without affecting the basic meaning of the sentence.

But where two inanimate or two obviative nouns co-occur, role allocation cannot be signalled by matching. In such cases, the actor will be represented as proximate in the verb, and sequencing between the nouns becomes a signal: the first noun will be taken as the actor. (6a) is an example of this kind. (6b) shows a comparable situation,

(6) Matching not possible.

(a) Two obviative (or two inanimate) nouns as actor and goal: 3S/P ACT required in the verb, and actor noun precedes goal noun.

/uda·nsan ugi·-ži?a·n niw bəkwe·žəganan/

| | | | |
|---|---|---|---|
| her-daughter | she-made-it | | the-bread |
| 3' | 3S | 3' | 3' |
| | ACT | PAT | |
| ($N_1$=ACT) | | | ($N_2$=GOAL) |

'Her daughter made the bread.'

(b) Two obviative nouns as possessor and possessed: 3S/P POS required in possessed nouns, and possessor noun precedes possessed noun.

/ugwisan udayan/ 'his son's dog'

| | |
|---|---|
| his-son | his-dog |
| 3S 3' | 3S 3' |
| POS, POSD | POS, POSD |
| ($N_1$= POS) | ($N_2$=POSD) |

where two obviative nouns occur as possessor and possessed. In "his son's dog", there are two possessed nouns, "his son" and "his dog", both nouns inflected for proximate possessor, obviative possessed. Again, it is the sequencing between the two which signals that the son (obviative) is the possessor of the dog (also obviative), rather than the reverse.

The apparent "mis-match" between a proximate verbal participant and an obviative noun, as shown in the examples of (5) and (6a), is a recurring and predictable phenomenon, both in Parry Island Ojibwa and, judging from Bloomfield's examples, in the Walpole Island dialect of *Eastern Ojibwa*. The situation is not one of "unpredictable neglect of obviation." I believe that it was Bloomfield's analysis of certain other forms as "further obviatives" which made cases like (5) and (6) seem anomalous. Clearly, if there *were* forms meaning "the other made it" and "the other's dog", as Bloomfield suggested, (6a) and (6b) could be expected to require them. But the forms in question (*ugi·-ži?a·ni* and *udayni*) are not allowed in these examples, and would in fact be nonsensical. The sentences in (7)

(7) *Matching not wanted: "somebody else".*

(a) An "actor-concerned" is different from the main (prox. or obv.) actor.

| /uda·nsan | ngi·-šamig | niw bakwe·žoganan | ga·-ži?a·njin/ |
|---|---|---|---|
| her-daughter<br>3' | she-gave-me<br>3S<br>ACT | the-bread<br>3' | "somebody else"-made-it<br>3<br>ACT-CONC |

'Her daughter gave me (some of) the bread that someone else had made.'

(This 'someone else' may be the proximate possessor -- the girl's mother -- or someone not mentioned here.)

(b) A "possessor-concerned" is different from the main (prox. or obv.) actor.

| /ja·n | ugwisan | noki·wan | ma· | ugitega·nning/ |
|---|---|---|---|---|
| John | his-son<br>3' | the other-works<br>3'<br>ACT | | in someone else's garden<br>3<br>POS-CONC |

'John's son is working in someone else's garden.' (The garden may belong to John or to someone not mentioned here.)

are typical cases where such forms are called for. In these sentences, substitution of ordinary forms without the concerned role changes the identifications. (7a) with *ga·ži?à·jin*, signalling a proximate *actor* rather than *actor-concerned*, now means "Her daughter gave me some of the bread she had made herself," and (7b) with *ugitəga·ning*, signalling a proximate *possessor* rather than *possessor-concerned*, means "John's son was working in his own garden."[5]

Decoding is cumbersome to describe. And yet, where cross-reference holds, as in (4), the mechanism itself -- the matching of the participants -- is an extremely simple one.[6] It is only the cases where cross-reference cannot apply which tend to complicate the picture. But here too, the language provides solutions. In sentences apparently calling for inflected forms involving two peripherals, an obviative noun may be identified with a proximate actor or possessor. And when several obviative nouns are involved, sequence becomes a signal for the first time. But where the identifications made in these ways would be the wrong ones for the speaker's intended meaning, special forms can be used to prevent the identification of a later actor (or possessor) with an earlier actor.[7] And if all else fails, one can always start again, putting some of the information into a new sentence, or beginning a new span of sentences.

## NOTES

[1] This sketch depends crucially on the work of other Algonkianists, particularly Leonard Bloomfield and C.F. Hockett. (Cf. especially Bloomfield, *Eastern Ojibwa: Grammatical Sketch, Texts and Word List*, Ann Arbor: University of Michigan Press, 1958; Hockett, "Implications of Bloomfield's Algonquian Studies," *Language* 24: 117-31, 1948; Hockett, "What Algonquian is Really Like," *IJAL* 32: 59-73, 1966.) My reinterpretations rest on a scheme developed in my dissertation (*Participant Identification and Role Allocation in Ojibwa*, Ph.D. diss., University of Toronto, 1973) and two recent papers on verb inflection (*IJAL* 41: 21-31 and 114-39, 1975). The "roles" employed in my description are rather like C.J. Fillmore's "deep cases," except that the roles postulated below are meanings signalled by particular elements identifiable in the texts of a specific language, in accordance with M. Joos's cryptic dictum that "text signals its own structure." The data described below come from sporadic fieldwork at Parry Island, Ontario, and with Parry Islanders resident in Toronto, the work extending over approximately the past nine years.

[2] A real difference between decoding and interpreting is suggested by cases where a hearer first rejects some utterance as nonsensical, and subsequently reverses this verdict on thinking of some context in which the utterance in question would after all be a sensible thing to say.

[3] Consider also the following, where a personal pronoun *ni·nwind* 'we, exclusive' occurs as orientor:

| ga·wi·n nda·-bi-ža·si· | ; | nda·kuzi·cəge· | sa | *ni·nwind* |
|---|---|---|---|---|
| I can't come over | ; | I have sickness | | "in the family"<br>(or) "at our place" |

(The particle *sa* appears to be untranslatable.) Since no other role is available for the pronoun, it can be identified as an orientor. The usage can evidently be compared to French "chez nous."

[4] The third constituent in (4) can be decoded as follows:

| we | - | gwis | - | Ø | - | an |
|---|---|---|---|---|---|---|
| 3S | | *son*, an. | | *P* POSD,<br>*C* POS | | 3' |

*son*, an.
*P* POSD, *C* POS
3' 3S

*son*-3' POSD, 3S POS
'his son'

Some noun stems require {-em-} rather than "Ø" theme sign, with the same meaning.

[5] For a sentence "John was working in someone else's garden," cross-reference must be avoided: /ja·n ugi·-noki· ma· *ugitəga·nning*/. In the current approach, *ugitəga·nning* is analyzed as follows:

we - gitiga·n - Ø - ini - ing
3S garden, in. P POSD, 3 C CONC InL ORI
C POS
(a) (b) (c) (d) (e)

(b) garden, in.
(c) P POSD, C POS
(e) InL ORI
(d) 3 C CONC
(a) 3S

garden-ORI-POSD, 3S-POS-CONC
'in someone else's garden'

In this sentence, Bloomfield's analysis 'in the other's garden', with obviative possessor, would evidently fit the facts quite as well as the current analysis. But the traditional analysis cannot account for the required absence of such forms in cases like (5) and (6), or the significance of their presence in cases like (7).

6 The explicit coding shown for examples (3) and (4) involves essentially the same "mechanism". Within a single transitive form like (4), the roles are allocated to appropriate participants through congruence, shown by the dotted lines in the diagram. And within a sentence like (4), cross-reference allows the verbal roles to be allocated to the appropriate nouns through matching, shown by the solid lines of this diagram. In both cases -- for a single inflected form and for a whole sentence -- one mechanism seems to be (repeatedly) involved.

This mechanism can be expressed in abstract form as $[ab_1 + b_2c = a(b)\ c]$, where $b_1$ and $b_2$ are congruent or identical meanings signalled by different grammatical constituents. The apparent repetitiveness is not simple redundancy. On the contrary, it is what allows "a" to be correctly associated with "c". As inspection of the coding diagrams suggests, within a single inflected form, "a" and "c" are a role and a participant, linked by recurring categorial elements which cannot be further analyzed into smaller components. And in a sentence, "a" and "c" are a role and the lexical content of a noun, linked by recurring participants which are categorially complex.

7 My data show one example of what may be a different sort of "escape hatch." It involves the use of a nonspecific alternate verb form, one which specifies a peripheral animate goal, without identifying the actual participant. The sentence in question is the following:

ndawi-wa·boma·min ; mi· daš aw ga·-wa·boma·ngid
we were on our way to see someone ; but he was the one we saw.
P 3S

The proximate/obviative contrast cannot be used here, since there happen to be no verbs specifying obviative goals with first or second person actors in Parry Island Ojibwa. The (required) use of nonspecific 1P → P in the first verb and 1P → 3S in the second, a participle, evidently prevents an unwanted matching between the two goals. Since this is the only such case in my data, checking of this hypothesis must await further work. But it is interesting that Ives Goddard found two different uses for such "nonspecific alternates", one characteristic of Abnaki, and the other characteristic of Natick. (He calls them "absolute", in contrast to "objective" forms. Cf. Goddard, "The Algonquian Independent Indicative," Nat. Mus. of Canada, Bull. 214, p. 77.)

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# PHYSICAL ANTHROPOLOGY IN NORTHWESTERN ONTARIO: PROGRESS REPORT ON THE NORTHERN ALGONQUIAN PROJECT

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The primary objective of this project is to increase our understanding of human adaptation to the sub-arctic forest. While human adaptation is clearly an exceedingly complex matrix of interacting biological and behavioral patterns, the present approach emphasizes the biological. It represents the first intrusion of adaptive biological anthropology into this area.

One methodological and one theoretical focus make the project distinctive. Whereas the usual fieldwork pattern for adaptive human biology is characterized by the rapid in and out visit, we have been able to place trained biological observers in two small native communities for a year each. The gains in depth and richness of knowledge are well recognized within anthropology; supplementary studies have been conducted during shorter field visits, and, through the collaboration of ethnologists. Secondly, we hoped to concentrate our efforts on cold adaptation, both biological and behavioral. Differential (cross-regional and intrapopulational) capacity to deal with life at low temperatures has developed as a theoretical issue within physical anthropology. Elsewhere, there appear to be physiological as well as anatomical adaptations to cold which may be partly genetic in origin; this implies they arose as a result of natural selection. Consequently, we hoped to test northern Algonquians for the presence of such special traits, and to look for hints of ongoing selection.

## Research Sites

By design, the communities studied ranged across the zone of transition between the heavy coniferous forest and large, deep lakes of the Severn uplands on the south, to the near-tundra where the lowlands ease into Hudson Bay. They lie in the drainage basin of the Severn River as it flows northeastward into the great salt water bay (see Fig. 1).

Farthest north are the villages of Winisk and Ft. Severn; both are coastal communities at the mouths of major rivers. Marshall Hurlich resided in Ft.

Severn for a year between fall of 1973 and summer 1974; a "working" trip was made to Winisk. During the 1975 calendar year, Bruce Winterhalder lived with the people of Muskrat Dam--a site south of the transition between lowland and uplands. And finally, some time was spent at the most southerly site, Weagamow (Round Lake). The writer was there for the coldest part of the 1974 winter looking at biological cold adaptation, and other contributions are coming in from the ongoing work of Edward S. Rogers and Mary Black, and from Louis Marano who permanently resides there.

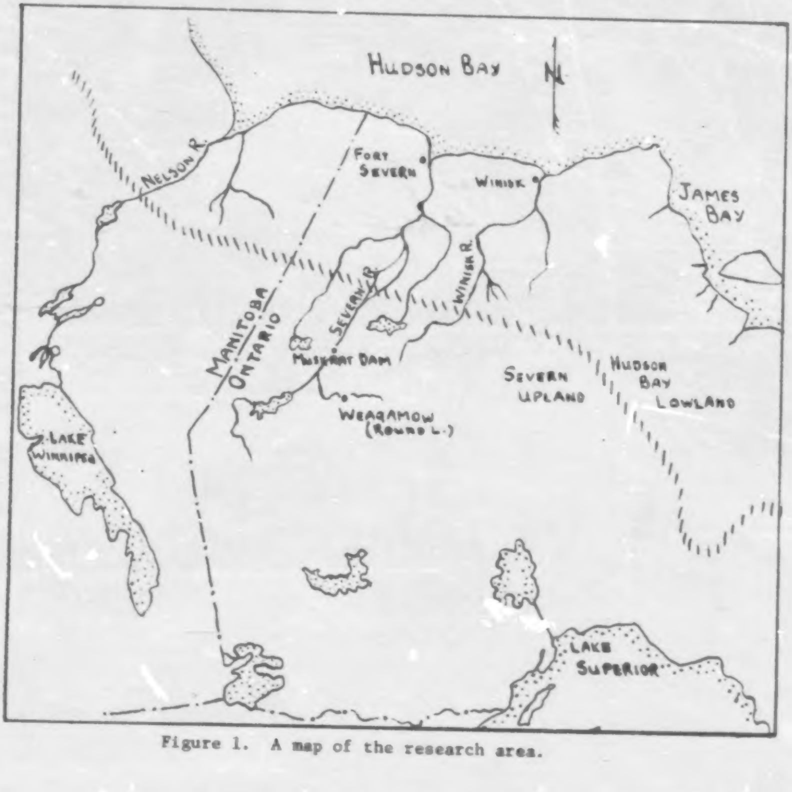
HUDSON BAY
N
NELSON R.
FORT SEVERN
WINISK
JAMES BAY
MANITOBA
ONTARIO
SEVERN R.
WINISK R.
MUSKRAT DAM
WEAGAMOW
(ROUND L.)
SEVERN UPLAND
HUDSON BAY LOWLAND
LAKE WINNIPEG
LAKE SUPERIOR

Figure 1. A map of the research area.

## Research Topics and Results

Demography. Humans adjust to environmental pressures and limitations in both obvious and not-so-obvious ways. Among the most obvious and readily analyzed are the means of maintaining the "proper" number of people that fit in with resources. Demographic description, then, was a first and most attractive problem, not only for its adaptive significance but also because comparable data are widely available from other northern settlements and from elsewhere in the world.

Unlike information available for other one-time nomadic hunters, there is here a fairly long historical record. In at least two communities, demographic patterns obtained through collection of oral geneologies can also be carried backward through records kept by the Hudson's Bay Company, churches, hospitals, and the Department of Indian Affairs. By listing numbers of community residents according to age category, population stability, decline, or growth can be determined. Future growth and size can be predicted, and especially, social, economic, and political factors which influence population characteristics can be sought. To the physical anthropologist, the movement of people into a population may result in gene flow as well as cultural diffusion, and this is one of the problems with which we are concerned. Likewise, recording the loss of group members either by emigration or death can be very helpful in understanding population dynamics. In fact, information on illness and death serves the additional purpose of trying to decipher how much biological selection has resulted from "natural" forces such as cold, accident, and disease, and how much from social causes such as homicide.

We hope eventually to have detailed comparative information on demography from Ft. Severn, Muskrat Dam, and Weagamow but can already report observations from Ft. Severn as condensed from geneological interview and written records. The present community was consolidated out of Cree groups from the Hudson Bay coasts of Ontario and Manitoba, plus some people from inland areas. It may or may not represent an aboriginal mendelian population, or "Dialect Tribe" (Birdsell 1968). Between 1944 and 1959, the population size was relatively stable, but the pyramid base is now very broad, especially due to the large numbers of youngsters 15 years old or less. Consequently, strong numerical growth is predicted, unless offset by emigration. Numbers of people leaving have indeed been increasing, and endogamy has apparently been declining as well.

Using geneological reports only, a sizeable amount of material on mortality was accumulated. As one would predict, in most cases cause of death was uncertain. However, about one third of deaths were said to be from disease, and to my surprise, only about ten percent from such "environmental" causes as freezing and drowning. From these, a glimmer of insight into selection was found. Unfortunately for tests of selection in human populations, not all death is necessarily "selective"; it takes people who are past reproductive age, may not depend on genetic predisposition, and so on. We will still be able to make some estimates of current selective pressures.

Cold Response. Our perceptions about the problems and injury resulting from cold might have been different from those of people who really work at low temperatures. So, interviews were conducted, particularly at Ft. Severn, with children, women, and men in hopes of learning those aspects of work in the cold which they found most difficult. Women had few complaints in this area, but men reported that trapping and fishing were particularly stressful, as were snowmobiling (a new skill), outside maintenance, and hunting; children were uncomfortable when riding on tobaggans (behind snowmobiles) and at such tasks as hauling water. It would seem that loss of aboriginal nomadism has had the strongest effect upon the work situation of women.

In an effort to quantify cold response and to decide just how cold, for instance, are "cold hands," we measured hand and face skin temperatures thermoelectrically under a variety of field conditions. These ranged from single observations of one man hauling up fishnet to twenty men working together clearing forest. It is evident that working barehanded in water (often seen in winter fishing and trapping) produces dangerously low hand temperatures. However, frostbite was seldom more serious than first degree, and most people seemed to regard cold as not much more than a low grade, if potentially dangerous, nuisance. A considerable body of behavioral-thermal data will eventually be digested and reported, and we consider this one of our strongest areas of biological/ethnographic relationship.

Such tests on people doing their routine work were supplemented by others in which activities were simulated by subjects paid specifically for the job. These tests allowed us to collect much more data under conditions closer to controlled experimentation. For instance, ten men would snowshoe over a course as well as spend time standing with and without mitts. Exercise was an easy method of maintaining high body and hand temperatures, while standing barehanded resulted in the most rapid finger cooling. This may explain why most manual tasks were conducted with such speed during the winter, and we will eventually report many other behavioral correlates as well.

We also collected "laboratory" tests of people seated inside with hands in $5^0$ Celsius water. Since this is a standard sort of cold response test the world over, we will not only obtain Cree-Ojibwa comparisons, but also data on Indian/non-Indian responses and physiological correlates of hunting or trapping success. Whether laboratory results are predictive of hand temperatures seen in the same people under field conditions is an additional problem and data are now being analysed. Finally, anthropometric measurements were taken, especially for the purpose of discerning how well northern Algonquians fit world patterns of morphological variation. For instance, Roberts (1973) has shown that people native to cold regions tend to have relatively long trunks and short legs. The men of Ft. Severn do not conform to this regression closely, being "overly" long-legged. This may relate to recency of arrival, a snowshoeing adaptation, or various other reasons which will be pursued elsewhere. The theoretical underpinnings of cold adaptation studies are reviewed by the writer elsewhere (see Chapter 7, Damon 1975).

Resource Management. A major emphasis of Winterhalder's research at Muskrat Dam, and one on which Hurlich also collected a sizeable amount of data, was upon cultural and individual patterns of resource use. Included was consideration of local and external resources of both natural and cultural natures. Traditional techniques such as hunting and snaring were considered along with more recent methods of food raising and house building. Vegetation mapping by airphoto will aid in determining what type of zones were used, at what time, and how decisions for use were made. Differential use by age groups and by different families appears to be a research direction of some promise, but most of these data are so recently collected as to have not yet been analysed. We hope a clear picture of total community adaptation to this unstable but resiliant habitat will emerge in the end.

## Conclusion

The peculiar and knotty problem of interactions between man as a biological being and as a social and cultural actor is most easily examined in relatively simple, harsh environments such as the great boreal forest. For the current project we have focused on those biobehavioral phenomena which we assume to be adaptive. We feel useful information combining demographic, physiological, ecological, and ethnographic insights has been collected, and look forward to being able to report these in richer detail during future Algonquian Conferences.

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# THE SOLON PETROGLYPHS AND EASTERN ABNAKI SHAMANISM

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The Kennebec River was one of two primary river courses used by the Indians of Maine, the other being the Penobscot River. The drainage basins of these two great rivers comprised most of the territory of the historic Eastern Abenaki, the only remaining resident branch of which is the modern Penobscot tribe. At one point in southern Somerset County, a ledge of metamorphosed shale juts out into the Kennebec, forming a natural dock for canoes and, as it happens, a relatively smooth surface for petroglyphs. The site is one of only a handful of petroglyph sites in Maine, and the only one on which the glyphs are well preserved. There are about ninety distinct petroglyphs in a band eight meters long and two wide. Several more glyphs are too faint to be traced accurately, and the original number may have approached two-hundred.

The most striking characteristic of the petroglyphs when taken as a whole is their heavy sexual content. The only similar phenomenon that has been reported in detail is the petroglyph site near Peterborough, Ontario. Joan and Romas Vastokas have studied this site carefully, and have come to many of the same conclusions reached in my study of the Solon petroglyphs. Both sites lie within the general distribution of Algonquian languages a widespread and ancient language family to which we can safely assume the carvers of the petroglyphs belonged. The Vastokas' abandoned superficial explanations of the Peterborough petroglyphs early on in their investigations, just as I did. All of the standard explanations are palpably inadequate to explain the extraordinary glyphs at these sites. Like the Vastokas', I plunged into the literature on Algonquian shamanism. They found their most solid leads in the writings of Henry Schoolcraft. My own search led me to a renewed examination of the writings of Fannie Hardy Eckstorm and Frank Speck on Penobscot shamanism. In particular, I was drawn to the biography of John Neptune, the last great Abenaki shaman, who lived from 1764 to 1865. I was also led to a reappraisal of the rather large body of recorded Penobscot folklore.

The Penobscot shaman was called *medeolinu*, 'supernatural man.' Such a person was by definition the possessor of a vaguely defined but nevertheless potent magical power. Shamans were usually men, but occasionally a woman acquired the necessary power. In either case, acquisition of the power was involuntary. A shaman was regarded with a respect born of fear, and for reasons we shall see, this was particularly strong in the case of female shamans.

A shaman could cause illness and, in the sense that if he desisted the illness would disappear, he could also cure it. But the Penobscot shaman was not truly a curer. Herbal remedies and other non-magical curing techniques were normally the province of curers that did not possess magical powers. The curer had talent, but the shaman had a charisma that translated into prestige as well as both real and magical power. Thus, shamanistic power accrued to persons with strong personalities, the same people who usually rose to positions with strong personalities, the same people who usually rose to positions of leadership in this egalitarian society. In this way, secular power and sacred power reinforced and validated each other.

Another way in which the shaman's power was expressed and validated was through sexual prowess. Powerful leaders normally had two or more wives, and sexual reputations to match. This explains why in such a male-dominated society, female shamans were regarded as particularly threatening. John Neptune had three wives and a reputation as a libertine, the real significance of which ethnographers rooted in Victorian culture did not fully appreciate.

Penobscot shamans were quite independent, never coming together into organized societies like Ojibwa shamans. Each shaman had at least one animal helper into which he could transform himself when he wished. The shaman could move about in his animal helper's form. The shaman's body remained inert while this was happening, indicating that the shaman became the animal rather than simply sending out a helper, and if the animal happened to be killed, the shaman's body died in the same instant. Indeed this was the logical explanation when a known or suspected shaman died while either sleeping or unconscious.

Otters, beavers, porcupines, bears, eels, dogs, birds, and many other animals served as familiars into which shamans could transform themselves. There are many stories like the one in which someone recalls wounding an animal and later encountering a man with an analogous wound. The logical conclusion reached was that the wounded man was a shaman who was wounded by the first man while in his animal form. Such things were thought to occur in dreams as well, taking us another step away from our modern concepts of reality.

Shamans were reputed to have the power to kill by pointing. This technique was not so much observed as it was assumed in retrospect after a sudden and unexpected death. Indeed, direct confrontations between shamans in their human forms were carefully avoided. Fights between shamans were usually believed to occur when one or both of them were in animal form.

John Neptune was supposed to have been seven animal helpers, the largest number known for any Penobscot shaman. His principal animal form was the eel, and he probably followed the usual practice of treating all eels and the other six species with considerable respect. He could assume any of these forms and attack other shamans in their respective animal forms. Stories of such encounters often have heroic proportions, and one can imagine the effect the observation of two animals fighting to the death would have on a lone Penobscot hunter who believed them to be transformed shamans.

There are several stories that point to a link between shamanism and rock. In one such story, a shaman in the form of a white weasel fights an opponent, throwing him repeatedly in such a way that he sinks into a stone ledge up to his knees. The weasel finally breaks off his opponents legs and kills him. Other stories concentrate upon the shamans own ability to walk about on rock, leaving footprints that are sometimes knee deep. Gluskabe, the greatest of all fictional shamans in Penobscot folklore, was capable of this and a great many other feats. Like many shamans, he could also pick up huge stones as if they weighed nothing.

A short excursion into Penobscot folk tales also helps an interpretation of the Maine petroglyphs. There are more than a half dozen culture heros in addition to Gluskabe, and each can be shifted in and out of a cycle of stories in a way that seems confusing and imprecise to modern readers seeking a clear and permanent structure. Similarly, the Penobscot traditionally believed that a supernatural creature or creatures lived in (or on) Mt. Katahdin, a peak that dominates the interior of Maine. 'Pamola' is frequently mentioned, and commonly described as a bird with a huge head and wings but without a body. At other times we are told that the creature is covered with hair, or has a body but no anus, or is a handsome young man. In fact there is no consistent list of distinct beings, or even a consistent description of one of them, something that may be frustrating to folklorists but not to the Penobscot. The point is that when dealing with the supernatural there is no need for either precision or permanence, because the entities described are capable of endless transformation. This characteristic of traditional Penobscot belief makes shambles of modern efforts to impose a rigid structure upon it. It must also be kept in mind in any analysis of petroglyph motifs.

Among the most easily identified motifs are four ithyphallic male figures. They are also the most obvious evidence of the connections between shamanism, sexuality, and petroglyphs at the site. All four are shown in profile, moving left to right, and three do so with the left arm raised. One figure has an enlarged left hand with fingers splayed, a specific symbol of shamans in Algonquian cultures. The same identifier can be found on some historic Ojibwa birchbark scrolls.

Specifically female figures are more difficult to identify, because anatomical clues are generally absent. Instead, women appear to be identifiable by posture. Three figures are shown with legs spread and knees drawn up, a potent gesture of sexual receptivity. There are several other human figures among the petroglyphs, but for these sex cannot be easily determined. Some seem to be incomplete, while others are lost in a confusion of compound forms.

Female forms appear more often as disembodied genitals. These occur in two basic forms, one an oval with a short interior line, the other a set of three lines each joined to the others at one end. The latter are not uncommin in North American petroglyph sites, but their frequent identification as bird tracks has tended to obscure their real meaning. Disembodied male genitals are less frequent. There are only two that can be positively identified, with two more possibly hidden within compound petroglyphs.

Of the two phalli, one is equipped with wings, such that it resembles bird forms found elsewhere at the site. There are at least fifteen birds among the petroglyphs, some of them almost human in form. At least three are ithyphallic, a sexual feature that is not characteristic of birds in nature. These forms are often called 'thunderbirds,' but such an identification unintentionally restricts their full meaning. For the Maine site, at least, the bird forms probably represent the supernatural 'Pamola' of Mt. Katahdin, interchangeable a man or flying creature, supernatural being or transformed shaman. Whether a phallic bird, a winged phallus, or a transformed human, it was probably all a single amorphous concept.

There are at least thirteen petroglyphs that represent canoes. Each usually contains two minimally human figures, and occasionally one or both are provided with long paddles. One is combined with a larger abstract form, another is inverted, and three others are linked together. Still another carries a complex superstructure with what may be an animal perched on top. Most, like the male figures discussed first, seem to be moving from left to right. At first glance, all appear to be straightforward, even mundane, in their meaning, perhaps the equivalents of Ojibwa water route markers. But there is no portage or stream fork at this location; nothing that would have required an aboriginal traffic sign. Instead, the meaning of the canoe motif probably parallels the others already described, a metaphor in which the distinction between transportation and transformation is blurred. The canoes with their stick-figure passengers were probably neither less mystical nor less powerful than the other figures.

Of the remaining petroglyphs, some are too abstract or eroded to allow identification, others are too simple to interpret. The simple H-form, for example, occurs at least twice here, and is also known for sites in Pennsylvania and Ontario, but its meaning remains obscure. Three house forms, whose shapes are consistent with known aboriginal house types, also defy interpretation. There are animals of unknown species, some of which resemble the 'signatures' of Abenaki sagamores on early treaties, possibly the primary forms assumed by these men in their roles as shamans. However, despite the remaining difficulties in interpretation, the fundamental connection between shamanism and rock art at the site in Maine and at many other sites is now generally accepted.

# THE ADAPTIVE ROLE OF WARFARE AMONG THE SOUTHERN ALGONQUIANS

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One of the cultural aspects of the Algonquian speaking Indians that caught the attention of the early European colonists was the proclivity towards warfare. Not a devastating or all-out warfare as we generally know it today but as A.L. Kroeber described it a warfare of constant attrition. Other workers in the field have described it as not only attritional but socially disruptive and one of the major reasons that there was no large-scale unity or political organization such as the Iroquois or Creeks experienced shortly after the arrival of the Europeans. In short, there has been a very negative interpretation advanced of the significance of the aboriginal warfare.

This negative interpretation of aboriginal warfare habits reflects, perhaps, the cultural biases of the anthropologists and historians working in the field, for undoubtedly we consider warfare to be a "bad" or negative cultural practice which we advocate eliminating from the Earth; a goal which we seem singularly unable to accomplish. In this paper I would like to depart from the common attitude towards warfare and see if, perhaps, there was a reason for the constant conflict to see if it did, in fact, serve a basic underlying purpose.

The scope of the inquiry is limited to the territory controlled by what has been termed the southern Algonquians, namely Virginia and North Carolina. The writings of the various men who were involved in the ill-fated attempt at colonization on Roanoke Island in the 1580's and the writings of Smith, Strachey, and Beverly, of Jamestown, Virginia are utilized to obtain an understanding of the aboriginal adaption to the environment and an understanding of how warfare was conducted and what it accomplished.

To understand the possibly positive functions of warfare among the southern Algonquians it is first necessary to understand the basic adaptation that these people had made to their environment. It must be remembered that environment in the anthropological sense includes neighboring cultures or groups as well as the physical environment, for other groups infringing on one's territory represent competition for scarce resources, and the greater the population the greater the competition for those resources.

The tidewater area of Virginia and coastal North Carolina were considered by Kroeber and Mooney to have been among the most heavily populated regions of North America north of Mexico at the time of European contact and this population pressure certainly exacerbated any problems the Indians encountered in obtaining food.

The basic subsistence base of the southern Algonquians at the time of European contact was one of a primitive agriculture that augmented a hunting and gathering base. Smith and Strachey estimated that the produce of the fields yielded perhaps as much as 25 percent of the years food while the remaining three parts came from the wild. Thus we see a situation in which agriculture played a considerable but certainly not major role in the subsistence of the people under question. For this reason the seasonal cycle and scheduling of resource exploitation was a compromise between utilization of wild food resources and time spent planting and harvesting cultivated crops.

The yearly cycle can be outlined as follows. In the Spring large schools of anadromous fish ran up the rivers to spawn. Mainly herring and sturgeon, these fish could be easily caught in weirs (traps) which could operate all day without any humans in attendance. This fish run occurred during February, March and April. At this time fields could be tilled and planted for all available labor could be engaged in the fields with only an occassional trip to the weir to get enough fish out of the traps to feed the populace. This was an extremely fortunate coincidence for it meant that nobody had to be taken away from work on the fields to obtain food for the workers in the agricultural fields. In addition Spring is a very bad time of year for hunting as deer (the primary prey) become very wary as they approach the season of dropping the fawns and they are solitary. With the end of Spring, however, a bad situation developed for there was an end to the fish runs and weirs are ineffective if the fish are not moving in a uniform direction in large schools. In late spring there are few if any wild plant foods ripe and edible, hunting remains difficult and in the northern part of the Atlantic coast there ensued a "starving time" which I am sure workers in the northern Algonquian literature are familiar with.

The southern Algonquians were once again blessed by their environment for they had access to large bodies of warm shallow waters in Chesapeake Bay and the North Carolina sounds. From these were gathered enough shell-fish, crustaceans, and miscellaneous fish to sustain the population over the "starving time". Since populations are limited by the worst season of the year, the season which provided little food and hence limited the population of the northern Algonquians was bypassed by the southern Algonquians, thus allowing a tremendous increase in the population over the levels encountered in the New England states. As we shall see, however, this may have been the cause of a larger problem.

By mid-Summer the starving time was over and the population returned to the fields to commence the gathering of the crops. This harvest continued through the summer and well into the Fall and included not only the cultivated crops but the wild crops that were gathered. These included wild rice, nuts, fruits, root crops such as China Brier (Smilax bona-nox) and other insundry plant foods. Winter saw the Virginia Algonquians move to the interior to take up a winter hunt while the North Carolina Algonquians seem to have remained on the coast and enjoyed plentiful hunting in their home territories. Early Spring saw the return of the anadromous fish and a new beginning of the yearly cycle.

A little cultural background is now in order to complete the picture of aboriginal subsistence. The fields were tended by both men and women who performed, however, different tasks. The men were primarily responsible for the clearing of new land and helped to a lesser degree with the reclearing of old land. Both men and women hoed the ground to prepare the fields for planting but it can be assumed that most of the work fell to the women for old fields were generally more important than new fields. New fields were cleared by notching the trees the first year and letting the trees die. The second year the trunks and roots were burned and the roots ripped out of the ground. Crops were then planted. A field had a useful life of four to six years after which small scrib made clearing difficult. Thus in any year only 1/5 to 1/6 of the fields were new, the remainder being old fields.

Once cleared, however, the fields became the domain of the women and children who planted and cared for the crops. The men did not participate again until the harvest when they aided in taking in the crops, although it is apparent that once again most of the work fell on the women. It would seem, then, that the women were primarily responsible for the work in the fields and were the limiting factor in the production of agricultural crops.

If the women were the primary agriculturalist then the men were, of course, the hunters. The major season of hunting was the winter months when deer tended to herd together and bears (a favorite meat of the southern Algonquians) were fat and somewhat lazy. Smith and Strachey describe the winter months as months of hunting, and significantly, warfare.

We now turn to a description of the aboriginal warfare. Smith described it as a matter of stealth in which the idea was to avoid a pitched battle but to take the enemy by deceit. Several specific instances were recorded of the

Indians of one village luring the men of another out for a general hunt then setting upon them suddenly, killing all who did not manage to escape. The object of this warfare on a superficial level was to avenge past wrongs and maintain honor. The primary booty of the raid was women and children who were taken back with the victors and kept as slaves or integrated into the group. In demographic terms, the victors eliminated men and increased the percentage of women in their own village. This was noted by Smith who saw that there were usually more women than men in any village.

Land was apparently not a prize of war for land tenure was by occupation and not by any claims. In other words, a group occupied all of its land and land not so occupied did not belong to them. Goods were also rarely prizes of war.

If we combine the data on subsistence with the data on warfare an interesting pattern begins to emerge. The last bit of information needed is to note that food was a limiting factor in Virginia and to a lesser extent in North Carolina. Smith noted that there were no deer in Tidewater for they had been hunted to extinction in that area and the populace was forced to move to the interior during the winter hunt to survive. Strachey noted that Powhatan, the chief werowance (leader), counted his wealth in food items, indicating some scarcity of the material. Thus there was apparently a shortage of foods at some times during the year although the season of scarcity is not noted.

Assuming a large population as evidenced by the records and assuming that the wild food resources were being strained as evidenced particularly by the lack of deer meat in the Tidewater area, it follows that the only way to increase the food supply was to either lower the population or increase the supply of agriculturally produced food. Both objectives were met by the style of warfare practiced by the southern Algonquians. First, the men, who were the hunters, were killed thus lowering the competition for the hunted animals. Since women did not hunt the elimination of men effectively lowered the pressure on deer and other hunted animals.

At the same time, the warfare resulted in the capture of women and children who were the principal producers of agricultural food. While we have no record of the percentage of grown food produced prior to the arrival of the Europeans it is somewhat logical to assume that the percentage must have been increasing with time. Indeed, as the supply of wild food is exhausted, or better, as the carrying capacity of the range is approached the production of food is a simple and viable mechanist to supply sufficient food for a growing population. Those villages that were successful in warfare increased their percentage of women and children and hence increased the amount of land that could be put to crops and thus the total amount of food available.

In addition, competition for hunted animal foods was reduced with the death of the hunters of the unsuccessful villages thus insuring a survival advantage for those villages that practiced warfare successfully. In this respect, warfare was an adaptive part of the total subsistence pattern, at least of the successful villages. The unsuccessful villages apparently faded into obscurity or remained for many years at a low population level which is, after all, a part of the total necessity of increasing food production and lowering population.

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# LEXICAL SYMBOLIZATION AND PHONOLOGICAL PROCESSES IN DELAWARE

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1. Introduction. The data that I wish to discuss in this paper is nothing particularly new. The grammatical structure and phonology of Delaware have been described in Ives Goddard's dissertation on Delaware verbal morphology (1969) and my own dissertation, which was a partial grammar of Delaware (1972). The main outlines of Delaware grammar are quite similar to the grammar of other Algonkian languages, and the phonology has many points in common as well. The purpose of this paper is only in part to describe unfamiliar data. A more important purpose is to address the question of how best to describe language data, a question that deals with the organization of grammar itself and therefore has broader ramifications for the description of human cognition and human experience in general.

By lexical symbolization I mean the process of converting ideas into spoken symbols--words or morphs--representing those ideas. By phonological processes I mean the modification of words or morphs so that the same unit may be pronounced quite differently in different contexts. Since language, like other forms of behavior, is patterned, it has seemed reasonable to linguists in recent years to describe language as rule governed. That is, the descriptions of language have attempted to use rules to define the possible arrangement of words and word parts to form sentences, and to specify the class of words that can be inserted into the various sentence positions that are set up by these rules. There are a great many questions about just how this should be done. My own assumption is that the rules are semantic in nature, following a model proposed by Chafe (1970a, 1970b). That is, the rules define possible semantic structures and somehow convert these semantic structures into arrangements of morphemes to form sentences. It is at this point that symbolization takes place--that formatives which are non-physical units of meaning are replaced by morphemes having phonological content--that is, containing sounds (or sequences of sounds) which of course are part of the physical world.

Now if we can imagine a language organized in such a way as to maximize the qualities of simplicity, efficiency, and logic, the process of symbolization in such a language would involve the one-to-one replacement of meaningful formatives by phonological morphemes, and that would be the end of it. Each morpheme would assume one form. Each morpheme would be invariable. There would be no phonological processes because each morpheme would always be pronounced in exactly the same way. Although a number of languages approach this ideal, no language achieves it completely. And in a morphologically complex language like Delaware (and other Algonkian languages) the task of accounting for phonological variation is a major undertaking.

What I would like to suggest is that the grammar of a language is organized in a way corresponding to the mental organization of the language in each speaker. In particular, this organization strives to maintain a one-to-one relationship between meaning and form. And when forms are modified in their pronunciation, this is accomplished by rules of several different types which are organized in a compact way and which modify the forms in distinctly different ways. While this view has much in common with notions that are widely held among linguists today, it is different in certain crucial details. For example, most linguists treat phonological rules as an undifferentiated sequence of rules, all of the same type, which apply in a linear order. My own view is that the different types of rules form subcomponents within the phonological component of the grammar. I believe that this is true of Delaware and other languages as well--perhaps of all languages--and if so, I believe we obtain insight into the way in which something quite complex like language (and the world of experience that it represents) is organized along relatively simple lines.

2. Morpholexical variation. Although the maximally efficient listing of lexical items would contain a single form for each meaning, the description of Delaware is simplified in certain cases by including in the lexicon alternative phonological specifications for certain forms. This is the simplest way to account for a number of morphemes which assume varying phonological shapes depending on their immediate surroundings. An example is the affix which denotes first person plural in verbs and possessed nouns. The lexical alternatives can be expressed as follows (vowels marked with a macron are underlying strong vowels):

$$(1)\quad \begin{bmatrix}\text{first}\\ \text{plural}\end{bmatrix} \rightarrow \left\{\begin{array}{lll} \text{-}\mathrm{n} & / & \mathrm{n\bar{e}}\_\_\_ \\ \text{-}\mathrm{n\bar{a}} & / & \_\_\_\# \\ \text{-}\mathrm{n\bar{a}n} & & \end{array}\right\} \begin{array}{l}(a)\\(b)\\(c)\end{array}$$

(1a) |n-lūsw-əkw-nē-n| → /nlu·su·k·ú·ne·n/ 'it burns us'

(1b) |n-kahēs-nā| → /nkahé·səna/ 'our mother'

(1c) |n-kahēs-nān-ak| → /nkahe·səná·nak/ 'our mothers'

It might be possible to choose one of these variants as the basic form and derive the others from it by rules. But if this approach were adopted, the rule would have to be set up to apply to a single morpheme. Such a rule is not really a rule at all, but simply a way of specifying that the morpheme in question is symbolized in varying ways--something that can be done just as easily in the lexicon. Specification of phonological variation as alternants within the lexicon can be termed morpholexical variation. The term was first used by Bloomfield (1939) and has been used by Goddard (1969) and Pearson (1972) but as far as I know has not been adopted by other linguists.

3. Morphophonemic variation. Once assigned phonological shape, formatives are subject to phonological modifications of three types. The first of these, variation of a highly idiosyncratic type limited to only a few morphs and involving a mixture of grammatical and phonological environments, can be termed morphophonemic variation. This term is commonly used nowadays to describe just this type of variation--that is, variation involving a mixed phonological and grammatical environment. The term has been used in the past to refer to any phonological variation which alters the shape of a morpheme, but the meaning I have in mind is much more specialized. The process can be

illustrated by a fairly simple example from Delaware. The description of Delaware transitive verbs is simplified if all such verbs are regarded as having a direction marker immediately following the verb stem. Since this direction marker does not occur before a certain class of transitive imperative markers, we can say that it is deleted by a morphophonemic rule, as shown below:

(2a) $\backslash\overset{1}{\text{pənaw}}\text{-}\overset{2}{\text{ā}}\text{-}\overset{3}{\text{əyēk}}\text{-}\overset{4}{\text{an}}\text{-}\overset{5}{\text{y}}\backslash$ → (2b) /*pənaw-əyēk-an-y/

Individual morphemes can be glossed as follows: (1) verb stem 'look', (2) direct action marker, (3) prohibitive marker, (4) second person marker, (5) prohibitive marker. The composite meaning is 'don't look at him!'

The form as modified morphophonemically remains a non-occurring form, simply a step in the derivation of the actually occurring form. Specifically, the second person marker normally supplied by the lexicon belongs to a set of markers which is replaced by another set in this grammatical environment. Three affixes are involved in this set. The variation in these affixes cannot easily be stated in the lexicon because the environment for the replacement is created in part by the deletion of the direct action marker, which is itself a morphophonemic process. Even if this obstacle could be overcome, it would still be necessary to state the environment for the change in three separate places. By treating the replacement as a single morphophonemic process involving three separate morphs, the environment has to be given only once. In the form cited above, the operation of this morphophonemic replacement has the output shown below:

(2c) /*pənaw-əyēk-at-y/

The modified form is a further step in the derivation but not the final step. The segment /t/, introduced at this stage, is palatalized as part of another morphophonemic process:

(2d) /*pənaw-əyēk-ač-y/

The form remains subject to further rules which we shall consider presently.

Since morphophonemic rules are partly grammatical and partly phonological, it is well to ask at this point whether such rules are indeed required in a grammar. Could the functions of the morphophonemic component be handled just as effectively in some cases by morpholexical statements and in others by phonological rules which incorporate grammatical information? We have already seen that the alternation of /-an-/ and /-at-/ is related to two similar alternations which occur in the same environment and that this environment need be given only once if the alternations are stated as a morphophonemic, rather than a morpholexical process. But would it be possible to describe the zero allomorph of the direct action marker as morpholexical, rather than morphophonemic, variation? And would it be possible to describe palatalization as a phonological, rather than morphophonemic, process?

In the case of the direct action marker, the alternation between /-ā-/ and /-0-/ could be handled either way. Goddard (1969:87) describes the environments for each in a way that suggests an analysis of them as morpholexical variants. But in fact his description could be incorporated into a formal grammar either as a morpholexical or morphophonemic statement. Either way, the listing of environments will be fairly complex. Since the non-zero alternant occurs most frequently, it can be taken as the basic lexical entry. This approach simplifies the lexicon but requires a subsequent morphophonemic statement deleting the affix in certain environments. Neither approach is "correct" in the sense that it can be shown to correspond to a known or presumed mental representation of the language. In fact, the likelihood is that different speakers internalize the grammar of their language in slightly different ways. The linguist's representation of this organization is therefore bound to be arbitrary and is more likely to be fixed by considerations of elegance (as determined by the individual linguist) than by any objectively determinable scheme for the mental organization of a grammar.

Similarly, the palatalization of /t/ by a following /y/ could be described as a phonological, rather than morphophonemic, process since it applies to all sequences of /t/ followed by /y/. But the language contains a similar process in which /t/ is palatalized by a following /i/ if a morpheme boundary intervenes (e.g. /me·t·awsíč·i·k/ 'they who live bad', in which /-i·k/ is a pluralizing affix) but not within a morpheme (e.g. /nu·wí·t·i/ 'come here, little one' from underlying |nūwī-tī-l|). If palatalization triggered by /y/ and /i/ are to be treated together, both must be presented at a stage in the rule sequence where rules containing grammatical information such as the presence of a morpheme boundary are allowed, i.e. in the morphophonemic component. The alternative would be to present two palatalization rules separately, one as a morphophonemic rule and the other as a phonological rule, or to allow grammatical information in all phonological rules. Again, the solution adopted by the linguist is more a matter of taste than a question of demonstrable fact.

4. Phonological variation. A large number of phonological processes in Delaware have the effect of modifying morphemes quite conspicuously. But because these processes are quite regular and occur in environments that can be described entirely in terms of their phonological properties, they can be regarded as a second type of phonological rule, quite distinct in their overall properties from the morphophonemic rules just described. These very general rules can be called simply phonological rules or ordinary phonological rules if there is any need to distinguish them from phonological rules of other types. To return to the example just cited, we can observe three phonological processes which apply: (1) ə → i /__y. (2) y → Ø /__#. (3) C → C· / V̆__. Since these three rules apply in different environments, they need not be ordered with respect to each other. In addition, there are processes relating to vowel length and word stress: (4) the last non-ultimate strong vowel in polysyllabic words is assigned stress--as is the single vowel of monosyllabic words containing a strong vowel, and (5) non-final strong vowels are phonetically lengthened unless they precede a consonant cluster. The rules as stated here omit reference to a number of factors which would be relevant in a complete grammar. As applied to the form in question, the outcome is a broad phonetic representation:

(3a) /pənawiyé·k·ač/ (=2a)

The above is a representation consisting of just those phonological segments and suprasegmentals required to maintain contrasts in the language. Such a transcription has traditionally been termed a phonemic representation.

Among other Delaware phonological processes are three rules whose step-by-step application can be illustrated quite simply. The rules are the following: (6) metathesis of /w/ with a following /n/, (7) mutation of /a/ to /o/ following /w/, (8) loss of /w/ before rounded vowels. Thus forms for the root meaning 'hand' undergo the following derivation:

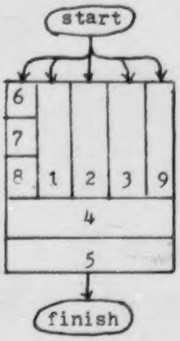
| (3b) | w-naxk | 'his hand' | k-naxk | 'your hand' |
|---|---|---|---|---|
| (6) | nwaxk | | -- | |
| (7) | nwoxk | | -- | |
| (8) | noxk | | -- | |
| | /noxk/ | | /knaxk/ | |

It should be clear that rules (6), (7) and (8) must apply in sequence. However, this set of rules is unrelated to rules (1), (2) and (3) since they apply in different environments.

Before considering the ordering relationship of these six rules to rules (4) and (5), one additional rule should be mentioned: (9) underlying weak vowels become strong if they precede a consonant cluster. This rule applies to the root vowel of 'hand' although its application is quite independent of all other rules except (4) and (5). Rule (9) must precede (4) since the noun 'hand' is stressed, and rule (4) must precede (5) since the root vowel in 'hand' remains short. The output is as shown in 3b except that the vowel is stressed.

The interplay of the nine phonological rules mentioned here can be represented as in figure 4. Any pathway from top to bottom is acceptable provided no vertical line is crossed. The fact that both rule (9) and the block consisting of (6), (7) and (8) sometimes apply to the same forms suggests that certain blocks of rules apply simultaneously or, at least, independently of each other. Independent pathways can of course merge--note that rules (8) and (9) both lead to rule (4)--and follow a single path from that point on. Further comments on the significance of rule ordering will be offered in the concluding section.

(4)



5. *Allophonic variation*. If we think of the output of phonological rules as a phonemic representation, we must still account for noncontrastive phonetic variation which occurs in specified phonological environments. A third type of rule component must be posited to account for variation of this kind. These rules can be called allophonic rules or late phonological rules. Either designation could be used, provided of course that the term is understood in the technical sense noted above.

A term like allophonic rules is quite descriptive, although non-structuralists may find it objectionable. I have previously described such rules as late phonological rules or LP rules (Pearson 1972). This term may be less objectionable, but it is open to misunderstanding since it usually refers to rules which apply late in a linear sequence of rules which are otherwise undifferentiated. It is worth repeating that whatever term is to be used must be understood as a technical term as defined above.

The output of allophonic rules is a narrow phonetic representation. A rather straightforward example in Delaware is the lengthening and nasalization of certain vowels before a nasal, with the accompanying loss of the conditioning nasal:

(5) /čú·ləns/ → [čú·lə̃·s] 'bird'

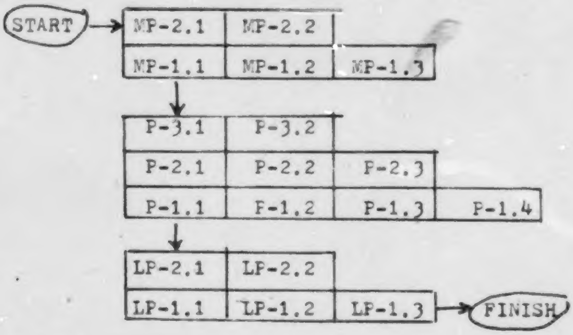
The tradition which has been established by Voegelin (1946) and Goddard (1969) for transcribing forms like 5 has been to use a broad transcription and provide allophonic rules by which the narrow phonetic representation can be derived.

6. Conclusion. To summarize, we can say that the process of symbolization and phonological modification in Delaware is accomplished in several distinct steps. A process of context sensitive lexical insertion provides the appropriate morpholexical variant at the outset in certain cases. A morphophonemic component modifies certain morphemes in special cases involving mixed phonological and grammatical environments. A phonological component makes adjustments that would normally be contrastive

but which are predictable in terms of phonological environments and therefore take place without actually affecting the meaning. Finally, a late phonological component specifies subphonemic variation.

Within each component there are certain rules which must apply in an ordered sequence. That is, they form a block in which they apply sequentially and which, as a block, may precede some rules and follow others. But within each component many rules may be quite independent of some or all of the other rules in that component. In other words, the rules within each component are partially ordered. The application of rules is something like that represented in abbreviated form in figure 6.

(6)



The rules can be thought of as applying from the uppermost layer to the lowest layer. Within each component rules in a given column are ordered with respect to rules above and below. But rules in the same row are unordered with respect to each other. The effect of an organization of this kind is to take what seems at first to be a hopeless tangle of phonological alternations and break them into a fairly simple series of related stops. Such a model is inherently more plausible as a scheme for human mental processing than a model which assumes a lengthy sequence of linearly ordered rules. The model given as figure 6 is in fact the kind of partially ordered rule sequence implicit in Chafe 1968 and Schane 1973.

The model of a phonological grammar as described here comes close to representing what a speaker does in using language. But it is important to remember that at best this model (or any model) can only come close; it cannot duplicate for an obvious reason. The capabilities of a linguist for storing and presenting information on a two dimensional sheet of paper are quite different from the capabilities of an actual speaker for storing information in a three dimensional nervous system.

This paper has been presented in terms of Delaware data, with the claim that the model proposed here provides an optimally simple description of the organization of different phonological processes. If it is true that all languages follow essentially the same organizational principles, it seems reasonable to suppose that the same model will also provide the optimal description of phonological processes in any other language. That, of course, is something that remains to be tested. Equally important and equally worth empirical testing is the claim that this kind of model, which breaks complex phenomena into simple, manageable steps, has a contribution to make to the understanding of human cognition and behavior in general.

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# A COMPARISON OF CHILDREN'S VALUES: TRADITIONAL CREE, ACCULTURATED CREE, EURO-CANADIAN

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## INTRODUCTION

The advent of large scale water regulation projects in the northern communities of Canada in recent times has brought the issue of pluralism to a peak. As dams are erected, waters backed up, and the ecology of reserve land changed, the question of the right of minorities to preserve their culture arises fresh in a most pressing and critical way.

During recent church-sponsored hearings, the Cree of Manitoba questioned the right of Hydro to threaten their way of life for the cause of meeting the growing electrical demands of the province (most especially the south). One Cree leader after another stood up to deny that they should be treated as though they were just like the people of the south, with the same values and needs and with the same potential benefits to be received from the extra electricity that would be generated. Hydro countered with the assertion that change is inevitable and that Cree cultural values are giving way to white cultural values.

The issue of the extent of the value differences between the northern Cree and the southern Euro-Canadian has theoretical as well as political facets. The provocative work of Hallowell (1955, pp. 351 & 357) and Spindler (1958 & 1971), which demonstrates how two Algonquian groups cling tenaciously to their traditional cultural values in the face of heavy acculturation pressure, suggests that acculturation need not produce value change. The conditions under which acculturation does or does not produce value change has been the subject of papers by Friedl (1956) and Boggs (1958). They assume that the acculturation situation has to be compatible with the traditional values if those values are to survive.

In an endeavor to throw some light upon these issues, this paper reports the results of a comparative study of the values of children in a traditional and acculturated Cree community and in a Euro-Canadian community. The data consists of the responses children make to a series of projective tests. This data is not meant to stand alone. It supplements other data derived from participant observation and interviewing. The data is suggestive, however, and certain implications from it will be drawn. These are to be treated as highly provisional and as having value mainly as leads into promising areas of further research.

## B. METHOD

### 1. Subjects

One hundred eighteen boys between the ages of eight and eleven were given a series of projective tests and a delay of gratification test. There were forty-four Euro-Canadians, forty-six acculturated Cree and twenty-eight traditional Cree. The three communities were Winnipeg, Norway House and Oxford House.

The Euro-Canadian boys were selected for study on the basis of an invitation from one of their teachers. All the boys in the third, fourth, and fifth grades who were given permission by their parents and teachers participated in the study (only a few boys were not given permission). There was an equal distribution of Ss in the three grade levels. The average age was 9.8.

The traditional Cree sample was obtained in two ways. Thirteen of the boys were contacted by a native researcher who tested them outside the school environment. Many were his relatives and were chosen on the basis of their happenstance availability at the times the researcher was active. The other fifteen were tested by the author in school. They comprised all the boys in the fourth and fifth grades who were attending school during the testing. In all, there were a total of five in grade three, eleven in grade four and twelve in grade five. The average age was 9.7.

## 2. Material and Procedure

Since there seems to be substantial agreement in the literature (Hallowell (1955, pp. 349-350), Spindler (1971, pp. 25-29), Caudill (1949), Friedl (1956), for example) that the traditional Cree psychological pattern or, more generally, the traditional Algonquian pattern is pragmatic and detail-oriented, pessimistic, anxious, aggressive, inward-looking and emotionally controlled, these were the psychological traits towards which our testing apparatus was sensitized.

Except for the traditional Cree children, all Ss were tested by the author in private during school hours, using the English language. Some of the traditional Cree children were tested in Cree by a native resident of the community who had been trained by the author.

Ss were taken out of classrooms and tested in a private room at a small table, the researcher and S facing each other across the table. The researcher followed a pre-set outline of sentence completions and thematic apperception tests. These were devised by the author to incorporate familiar cultural material and to eliminate Western biases. The sentence completions were read aloud first and the Ss replied aloud. Their responses were recorded in longhand. After the sentence completion, the Ss were shown 10 candid snapshots of Cree children and adults. Ss were asked to "describe what was happening." On occasion Ss had to be prodded by asking such direct questions as, "Why is she crying?", "What is he looking at?", or "What is he thinking?".

After the projective tests were administered, the Ss were told that the researcher would like to give them a candy bar for their help. They were asked, "Do you want to have one candy bar right now or do you want to wait a week without candy and then have two candy bars?" Those who delayed gratification got their candy bars a week later, those who did not, got a candy bar right away.

The testing took, on an average, about 25 minutes. The Ss were eager participants, although most of the Cree were shy as a result of their lack of confidence in English and their culturally dictated reticence. The researcher had to listen carefully for spoken answers which were often whispered behind upraised hands. But as they obtained confidence that the researcher understood their accent, they opened up and replied more quickly and freely.

Responses were scored blind by the author. Reliability checks by independent judges each scoring for a different personality variable showed 90% agreement with the author's scores.

An additional measure of psychological differences were obtained by the administration of a draw-a-man test to children of the same age and from the same schools as those who received the first test battery. There were 45 draw-a-mans from the traditional community, 53 from the acculturated community and 47 from the Euro-Canadian community. An independent judge analyzed this material. More detail on methodology is available in Hamer, Steinbring, Granzberg, et al. (1974 and 1975).

## C. RESULTS

The testing results are summarized under four headings: 1. emotional control and inward orientation; 2. particularistic orientation vs. holistic orientation; 3. pessimism and anxiety; and 4. aggression.

### 1. Emotional Control and Inward Orientation

Emotional control and inward orientation is indicated by drawings which show a man in limited contact with the surrounding world (e.g., by profile views rather than frontal stances, by lack of features and extremities, and by small size.) It is also indicated by a low need for achievement, by independence rather than dependence, and by supernaturally based rather than peer based answers to the question, "The man was bad so what happened to him?"

The results show that the average acculturated Cree child gives 7.6 closed, inward looking responses per drawing, as compared to 6.2 for the traditional child and 5.3 for the Euro-Canadian child ($x^2$=18.5; P<.001); that he gives a total of .3 need-achievement responses as compared to .4 for the traditional child (t=.43; P<.67) and .5 for the Euro-Canadian child (t=1.91; P<.06); and that he gives 5.5 dependent responses as compared to 4.6 for the traditional child (t=1.9; P<.05) and 6.4 for the Euro-Canadian child (t=1.7; P<.05). The results also show that 30% of the acculturated group forecast supernatural punishment (e.g., bad luck or sickness) rather than peer based punishment (e.g., being jailed or beaten up) in answer to the question about what happened to the bad man. This compares with 60% of the traditional group ($x^2$=4.53; P<.05) and 0% of the Euro-Canadian group ($x^2$=13.5, corrected for continuity, P<.001).

### 2. Particularistic Orientation Rather than Holistic Orientation

Particularistic orientation is indicated by a lack of original and imaginative responses to sentence completion items and pictures; by detailed descriptions rather than plots and holistic ideas; by an inability to delay gratification; by forecasting short range rather than long range goals in answer to the question, "A boy was walking in the bush and then he came to a stream, what did he do then?" and by detailing a part of the body that is sick in answer to the question, "The man was sick because ________?"

The results show that the average acculturated Cree child gives 2.0 imaginativ responses as compared to 1.2 for the traditional child (t=1.68; P<.10) and 3.1 for the Euro-Canadian child (t=2.0; P<.05); that he gives .77 detailed responses as compared to .92 for the traditional child ($x^2$=.72; P<.50) and .30 for the Euro-Canadian child ($x^2$=20.1; P<.0001). The results also show that 59% of the acculturated group do not delay gratification as compared to 92% of the traditional group ($x^2$=3.6; P<.10) and 32% of the Euro-Canadian group ($x^2$=13.2; P<.001); that 35% of them forecast short range goals for the boy who came to a stream as compared to 61% of the traditional group (t=2.10; P<.04) and 5% of the Euro-Canadian group (t=3.55; P<.001); and that 30% of them answer the question of why the man was sick by detailing a part of the body that becomes sick, as compared to 14% of the traditional group (t=1.58; P<.12) and 9% of the Euro-Canadian group (t=2.60; P<.02).

### 3. Pessimism and Anxiety

The acculturated group responds 67% of the time with pessimism rather than optimism. This compares to 78% for the traditional group ($x^2$=2.8; P<.10) and 54% for the Euro-Canadian group ($x^2$=4.5; P<.05). The average acculturated Cree child gives 1.6 anxiety responses compared to .9 for the traditional child (t=2.2; P<.04) and 1.5 for the Euro-Canadian child (t=.35; P<.74).

### 4. Aggression

The average acculturated Cree child gives 2.6 non-aggressive responses as compared to 2.9 for the traditional child (t=.67; P<.51) and 3.8 for the Euro-Canadian child (t=3.32; P<.001).

We may summarize these results in the following way: taking a probability of .05 as the level at which to reject the null hypothesis, the acculturated Cree children as compared to the Euro-Canadian children are significantly more emotionally controlled and inward oriented, more particularistic oriented, more pessimistic and more aggressive. There is no significant difference in anxiety levels.

As compared to the more traditional children, the acculturated Cree children are significantly more inward oriented, more anxious, more dependent, and more able to delay gratification when presented with a candy bar. There is no significant difference between them in particularistic orientation, pessimism and aggression.

In all, there is one comparison where the acculturated Cree respond to the tests more like Euro-Canadian children than traditional Cree children. There are six comparisons where they respond to the tests more like traditional Cree children than Euro-Canadian children.

## D. DISCUSSION

The projective test data suggests that the acculturated Cree have remained relatively stable over time in their psychological makeup. Participant observation data and interview data support this and, in addition, provide a hint as to how it might be possible for the Cree to maintain traditional values in the face of heavy acculturation pressure.

During the period of field work, the acculturated community entered upon a new era of acculturation. Television was introduced. The field worker maintained a close watch on the effects of TV upon the community. The most interesting finding was not any new behaviour patterns (though there were some) but the reinforcement and stabilization of old patterns. Rather than TV effecting the community, it seemed more the other way around. It was found that the community interpreted TV in a way that allowed them to utilize traditional values in adjusting to it. To them it was not new. It was, in fact, a very old institution. It was the shaking tent, albeit the white man's version. The white man, it was said, had stolen the idea from the Cree and now, as a result, they too could communicate with persons afar, bring them into their homes, and receive prognostications of the future from the dream-like content. Although the Cree now had to buy back that which was stolen from them and which was once free, they now could once more summon the powers of the world into their homes and learn important things from them. A tradition was revived. This example of how the Cree dealt with what most thought would be a very disruptive item in their culture, would seem to throw light upon the process by which "new" acculturation pressures may be rendered relatively harmless. The Boasians long ago understood the process. An item has a function, form and meaning which may vary independently. When an item diffuses from one culture to another, its form may stay the same while its function and meaning change in conformity to existing cultural configurations. Such has been the case with TV among the Cree. The process is also exemplified by Cree Pentacostalism in which persisting themes of vision quest, shamanism, and spirit possession may be perceived.

Other examples could be cited and yet, in today's world, testimonials to the power of ideas to shape reality seem to be few in number. Materialism has taken its toll.

Prior explanations of the persistance of Algonquian values (Friedl, 1956 and Boggs, 1958), for example, have assumed that material acculturation forces were causal and that values persisted only if they were not in conflict with the acculturation environment. Our data suggests that people can be very active in ensuring that this is the case. They can rationalize new items so that potential conflicts are explained away. They can shape reality to fit their needs.

It is hoped that this return look at Algonkian culture change data will serve as a timely reminder to policy makers and theory builders not to ignore the active, free-willing aspects of human behaviour in their work. It is hoped that this data will spur an effort to establish an appropriate balance between the perspectives of idealism and materialism.

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# THE GENERATION GAP IN MONTAGNAIS DIALECTOLOGY

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It has frequently been said that sound change is so gradual that it is not observable, and that one generation does not perceive any difference between the way they pronounce a language and the way succeeding generations pronounce the same language, or vice versa. Which of us, it is asked, can say that our parents or grandparents have systematic differences in pronunciation from our own, or that our children or grandchildren have such differences. Yet obviously such differences occur somewhere along the generation line, since all languages of whose history anything is known exhibit sound changes from earlier to later stages. Note that I am not referring to individual items, or to items or systematic differences that may derive from the fact that grandparents, parents, and children may speak different dialects due to educational differences, migration, or changes in rural to urban or urban to rural locations. Grandpa may have been raised on a farm in Nova Scotia while I am a university graduate living in Toronto; such differences in our speech as may exist are easily recognized and frequently commented on. Or Grandpa may have been raised in Toronto just like me, but he says /šedyuwl/ 'schedule', while I say /skedyuwl/, since his linguistic horizon is in one direction while mine is in another. But we both say /skiym/ 'scheme' and we both say /šist/ 'schist', so there is nothing systematic about our pronunciation differences.

Nevertheless, there are a number of studies what indicate that sound change can occur within the space of two or three generations, and that speakers contemporary with one another in speech situations where the factors of education, migration, or class play no part do in fact exhibit pronunciations indicating that sound change has taken place. One early mention of this (for which I cannot at present find the reference) is the rhoticization of intervocalic /s/ in Latin: an elderly gentleman in republican Rome is quoted as saying that when he was a boy words like /generis/ 'type' (genitive singular), and /gerō/ 'I carry' were pronounced /genesis/ and /gesō/. One of the best known modern examples is Labov's celebrated study of the diphthongs /ay/ and /aw/ in Martha's Vinyard (Labov 1963), where a 92-year old informant had what Labov calls a centralization index--that is, a measure of the frequency of pronunciation of the diphthong /aw/ as [əw]--of 0.10, i.e. almost non-existant, while his 60-year old son had one of 1.18, considerably higher, and a third informant 31 years old had an index of 2.11, showing a marked difference between generations in respect to this feature of pronunciation. Labov's study was not as simple and straightforward as I have presented it, since he was dealing with statistical frequencies, not absolute changes; was considering a feature whose beginning lay outside his observations, and which occurs in other dialects of English as well, notable among them the dialect of what has been called 'Heartland Canada'; and took into account considerations other than age, among them occupation, ethnic identity, and career expectations. Nevertheless, it does show what we may call sound change taking place in the space of three generations still alive and speaking.

Such age-graded differences showing sound change in progress exist in various dialects of Montagnais, the branch of Eastern Cree spoken in the southern tier of Québec and in Labrador. At last year's Algonquian conference, Ford (1975) noted one age-graded difference that exists in Betsiamites--perhaps the location where these differences are the greatest--that of the developments of Proto-Algonquian */šk/ and */šp/ as [x] and [f] in the speech of the younger members of the community. I intend to document that development, and to mention two others that Ford did not mention, and to show that some other developments also exist in other communities on an age-graded basis. Needless to say, my analysis is not exhaustive, and other differences of this type doubtless exist and are awaiting further comment.[1]

Unfortunately, I do not have the kind of data that Labov had for English--fairly extensive texts of a number of different speech styles, recorded and minutely transcribed--the kind that permitted him to express his results in a fairly sophisticated statistical form, so that my results must be presented largely in a simple tabulation, showing the feature present or absent in a person's speech, with some variation where this has been observed, and some simple counts of variant realizations where a choice is possible. Nevertheless, such data may point the way to further studies with hopefully enough material to allow for the statistical approach that such a situation calls for.

Over the last two years I have interviewed 45 different speakers representing 9 different communities. The interviews consisted of word lists ranging from about 90 to 250 items, with some paradigmatic sets when appropriate, presented to the informants in English, French, or, in the case of monolingual informants, in Montagnais, for their translation or response. The responses were recorded on tape for later analysis. The speakers have ranged in age from 12 to 87, and in appropriateness from fair to excellent. As might be expected, one easily observable age difference is in vocabulary, with older speakers having a far greater command of terms, especially for flora and fauna, than younger speakers. There are also observable grammatical differences, with older speakers making distinctions that younger speakers do not, and presumably vice-versa, although I have no examples of this latter. As for the former, one grammatical distinction I have observed is in Maliotenam, where older speakers maintain a morphophonemic distinction in the obviative form for the word for 'canoe': 'my canoe' /ntu:š/, 'he sees my canoe' /ntu:tnu/, with a variation /š/ ~ /t/ as the stem-final consonant, while younger speakers have generalized the proximative form ending in /š/ to obviative position: 'my canoe' /ntu:š/, but 'he sees my canoe' /ntu:šnu/. This is not a sound change, since there are no other examples of /t/ → /š/ before /n/, but is in response to the universal linguistic tendency to simplify and regularize irregular paradigms by means of analogy.

Turning to sound change, it is in the dialect of Betsiamites, as mentioned above, that the generation gap is most clearly observable. I interviewed 6 speakers of this dialect, aged 21, 22, 25, 53, 55, and 85. Three--those aged 21, 53, and 55--are women, the other three men. (Incidentally, I observed no discernable differences ascribable to sex, although there may well be some.) In the treatment of the */šk/ and */šp/ clusters of Proto-Cree and (Proto-Algonquia */xk/, */θk/, and */xp/ have fallen together as /šk/ and /šp/ throughout the Montagnais-speaking area, excluding the further developments in Betsiamites). the two oldest speakers--those of 55 and 85 years of age--have maintained these clusters as such in those positions where the three youngest speakers have reflexes of /x/ and /f/ respectively. The following are examples of */šk/ clusters, all of which went to /x/ in the younger dialect in positions other than before a front vowel:[2]

| | Proto-Cree | Older | Younger |
|---|---|---|---|
| 1) my liver | neškwani | niškwan | nuxwan |
| 2) my leg/foot | neška:ti | niška:t | nixa:t |
| 3) my wife | neteškwe:ma | ntiškwe:m | ntixwe:m |
| 4) bear | maškwa | maškw | maxw |
| 5) fire | eškote:wi | škute:w | xute:w |

The following are */šp/ clusters that went to /f/ in the younger dialect before a front vowel, remaining /šp/ in the older dialect:

| | Proto-Cree | Older | Younger |
|---|---|---|---|
| 6) my arm | nešpetoni | nišpitun | nafatan |
| 7) my side | nešpike:kani | nišpitse:kan | nifitse:kan |
| 8) my back | nešpeškwani | nišpiskwan | nafaxwan |
| 9) up | ešpimenki | špmit | fmit |
| 10) when | ta:nešp- | ta:nišpiš | ta:nifis |

The following are */šp/ clusters that went to /x/ in the younger dialect before other than a front vowel, remaining /šp/ in the older dialect:

| | | | |
|---|---|---|---|
| 11) pipe | wešpwa:kana | ušpwa:kan | uxwa:n |
| 12) it snows | mešponwi | mišpun | maxwan |
| 13) window | -- | špwa:pa:n | xwa:pa:n |

It can be speculated that the */šp/ clusters before other than a front vowel went through a stage where they were */šk/, then fell together with the original */šk/ clusters in this position, becoming /x/. There is no evidence for this stage in the material I collected at Betsiamites, but in the dialect of Maliotenam, a 13-year old informant renders the word for 'it snows' as /miškwun/, and the word for 'pipe' as /uškwa:n/, exhibiting the missing stage. He has no further developments in his speech in the direction of the developments at Betsiamites, but it would be worth finding out what he says when he's 25.

To round out the picture, the following are examples of Proto-Cree */šk/ before a front vowel, where it went to /s/ in all dialects, both young and old:

| | | Proto-Cree | Older | Younger |
|---|---|---|---|---|
| 14) | my eye | neški:nšekwi | niši:šukw | nisi:hukw |
| 15) | kettle | aškehkwa | sukw | sukw |
| 16) | shoe | maškesini | masin | masiń |
| 17) | earth | aškyi | si | si |
| 18) | all | kaškilawi | kasinu | kasnu |

It is worth noting that although in the speech of the older speakers the treatment of the clusters is perfectly regular, and that both speakers have the expected reflexes, in the speech of the younger speakers there are inconsistencies, both in the expected reflexes, and between the speakers themselves. In the following items, the 21-year old has the expected young reflex /x/, while the 25-year old has the old reflex:

| | | Proto-Cree | 21-year old | 25-year old |
|---|---|---|---|---|
| 19) | my elbow | netuškwa:ni | ntuxwa:n | ntuškwa:n |
| 20) | sky, cloud | wa:škwi | wa:xwe:w | wa:škwe:w |

and in the following two items, the 25-year old has the old reflex, while the words are unknown to the 21-year old:

| | | | | |
|---|---|---|---|---|
| 21) | beaver | ameškwa | -- | miškw |
| 22) | goose | neška | -- | niškw |

The most likely explanation for at least the last two items is that they are vocabulary items that are not found in the younger dialect, and have been borrowed from the older dialect as such. This is supported by the fact that the 22-year old has /miškw/ for 'beaver' and /niškw/ for 'goose'.

Another feature that differs in the speech of older and younger speakers is the treatment of Proto-Cree */š/, deriving from Proto-Algonquian */š/, and the clusters */?c/, */ns/, */hs/, */?s/, */nš/, */hš/, and */?š/. In the older dialect this is /š/ in all positions; in the younger dialect, the /š/ has gone to /h/ initially and intervocallically, but remains /š/ in other positions, most notably word final, and of course in clusters with /k/ and /p/ where these clusters are preserved. The following are examples of older /š/ corresponding with younger /h/:

| | | Proto-Cree | Older | Younger |
|---|---|---|---|---|
| 23) | my eye | neški:nšekwi | nisi:šukw | nsi:hukw |
| 24) | my fingernail | neška:šya | niška:ši | nixa:hi |
| 25) | my grandfather | nemehšo:m | numušu:m | nimuhu:m |
| 26) | duck | ši:?ši:pa | ši:ši:p | hi:hi:p |
| 27) | skunk | še:ka:kwa | še:ka:kw | he:ka:kw |

The preservation of final /š/ in the younger dialect is illustrated by the following:

| | | | | |
|---|---|---|---|---|
| 28) | my daughter | neta:nehša | nta:nš | nta:niš |
| 29) | fish | name:?ša | name:š | name:š |
| 30) | rabbit | wa:pošwa | wa:puš | wa:puš |
| 31) | flint | manθehši | matiš | matiš |
| 32) | flesh | wiya:hša | wiya:š | wiya:š |

Again, the older speakers are consistent, while the younger speakers show some hesitations and inconsistencies. In the following item, the 21-year old has final /h/, while the 25-year old and the 22-year old have expected /š/:

| | | Proto-Cree | 21-year old | 22- and 25-year olds |
|---|---|---|---|---|
| 33) | canoe | o:ši | u:h | u:š |

and in the following, the 21-year old and the 22-year old have initial /h/, while the 25-year old varies between /š/ and /h/:

| | | Proto-Cree | 21- and 22-year old | 25-year old |
|---|---|---|---|---|
| 34) | river | ši:po:wi | hi:pu, hi:pi | hi:pi, ši:pi |

The 22-year old, interviewed the year previously, has a number of items in which he varies between /š/ and /h/, among them 'my eye' /nsi:hukw/ ~ /nsi:šukw/, and 'my grandfather' /numušu:m/ ~ /numuhu:m/, so this type of vacilation is apparently part of the younger dialect.

The true confusion, however, exists in the speech of the 53-year old informant. She combines the systems of older speech and younger speech, both between individual items, and having variation between the older form and the younger form in the same item. In her dialect, the word for 'my eye' is both /nsi:hukw/ and /nsi:šukw/, the word for 'my nephew' both /ntu:him/ and /ntu:šim/, and the word for 'duck' both /hi:hi:p/ and /ši:ši:p/, the word for 'skunk' both /he:ka:kw/ and /še:ka:kw/. The word for 'my throat' is both /nikwatiškwu/, with old /šk/, and /nikwatixwu/ with young /x/; the word for 'my niece' is /ntu:himxwe:m/, with young /h/ and /x/, but the word for 'woman' is /škwew/, with old /šk/. Nevertheless, a rough count of the 227 items recorded shows that where there is a choice, she chooses the old form 42 times, and the young form 16 times, putting her statistically on the side of the older speech. It is interesting to compare her with the 55-year old, also a woman, who has the old form in all items. I did not investigate all the sociological parameters present in the speaker's backgrounds, but casual observation would indicate that it should be the other way around: the 53-year old, who mixes the young forms with the old, spent a good deal of her girnood in the woods, has little education, and is functionally a monolingual speaker of Montagnais--she understands some French, but does not speak it; the 55-year old, who has the old forms only, did not spend time in the woods, is well-educated, and is quite fluent in French. We must assume that there are attitudes towards the society in general that dispose the speakers to other than expected stances in regards to their usage of Montagnais, but these remain to be investigated. It is unlikely that age alone causes the discrepancies in the expected pattern, but it is undoubtedly one of the factors.

Ford has also pointed out that one characteristic of the dialect of Betsiamites is the apocopation of an initial /a/ in a number of items where the /a/ is retained in other dialects. Again, this is apparently an age-graded feature at Bestiamites. The retention of the /a/ occurs only in the speech of the 85-year old informant, and even then not in all the places where it might be expected. The following items show an initial /a/ in his speech and its lack in the speech of the next oldest informant, the 55-year old:

| | | Proto-Cree | 55-year old | 85-year old |
|---|---|---|---|---|
| 35) | dog | aθemwa | atim | tamw |
| 36) | beaver | ameškwa | amišk | miškw |
| 37) | caribou | atehkwa | atuk | tukw |
| 38) | snowshoe | aša:ma | aša:m | ša:m |

| | | Proto-Cree | 55-year old | 85-year old |
|---|---|---|---|---|
| 39) | kettle | aškehkwa | sukw | asukw |
| 40) | earth | aškyi | si | asi |

Although it may derive from a different historical process, an initial vowel, /i/ or /a/, occurs in the 85-year old's speech in the following two items, being missing from the items in the 55-year old's speech:

| | | | | |
|---|---|---|---|---|
| 41) | fire | eškote:wi | škutew | iškutew |
| 42) | up | ešpimenki | šmit | ašpmit |

Items in which the 85-year old, like the other speakers, has no initial vowel in the expected place are as follows:

| | | | | |
|---|---|---|---|---|
| 43) | mink | -- | tsika:š | tsika:š |
| 44) | bow | ahta:pya | tsa:pi | tsa:pi |
| 45) | arrow | akaškwa | kiškw | kiškw |
| 46) | arrowhead | aθwehši | tiš | tiš |
| 47) | who | awe:na | we:n | we:n |
| 48) | he works | aθoškye:wa | tase:w | tase:w |

It is possible to speculate that perhaps when the 85-year old was himself 53, he found himself in a position to the 53-year old of today, half-way between generations that were changing the language, and adapted in a similar manner.

It is worthwhile to see what features of dialects outside Betsiamites exhibit similar age-graded variation. The most outstanding is the change of /š/ to /h/ in the dialects of the Lower North Coast of the St. Lawrence. In these dialects, the change is apparently complete in the speech of younger speakers, as in /hi:bu/ 'river', /numuhu:m/ 'my grandfather', and /nimih/ 'my sister'. But among older speakers, the /š/ is retained in at least some of the cases. I interviewed speakers of various ages in three communities on the Lower North Coast: Mingan, Natashquan, and La Romaine. The following table shows the number of times /š/ or /h/ was used at Mingan:

| | Speaker's age | /š/ | /h/ | |
|---|---|---|---|---|
| Mingan | 87 | 2 | 22 | Damien Mistokosho |
| | 74 | 3 | 28 | Madelaine Mistokosho |
| | 66 | 5 | 12 | Philomène Bérnard |
| | 72 | 15 | 24 | Agathe Mistokosho |
| | 67 | 25 | 11 | Sylvestre Mollen |

The first three used a preponderance of forms with /h/; the last two used a higher percentage of forms with /š/. However, the last two are not natives of Mingan. The 72-year old is originally from Natashquan and has lived in Mingan for approximately 40 years; the 67-year old is from La Romaine, and has lived in Mingan for approximately 50 years. One might speculate that these two speakers had grown up in a time and place where /š/ was retained, and, due to change in residence and the feeling of being in some way an outsider, had kept that feature in Mingan, where, as evidenced by the high percentage of use of /h/ by the 87-year old, the change had taken place some time ago. This would indicate that the change had taken place earlier in Mingan than it had in either of the other two communities.

The following table shows the number of times /š/ and /h/ were used at La Romaine:

| | Speaker's age | /š/ | /h/ | |
|---|---|---|---|---|
| La Romaine | 70 | 8 | 22 | William Mark |
| | 66 | 5 | 14 | Philomène Mark |
| | 48 | 5 | 15 | Barthélemy Mark |

Here speakers comparable in age with the 67-year old in residence in Mingan have a preponderance of forms with /h/, but are in their own communities and have probably changed as the community changed.

The following table shows the number of times /š/ and /h/ were used at Natashquan:

| | Speaker's age | /š/ | /h/ | |
|---|---|---|---|---|
| Natashquan | 78 | 19 | 13 | Michel Gregoire |
| | 68 | 19 | 5 | Pierre Tubie |
| | 42 | 36 | 7 | Abraham Malec |

At first blush it looks as if /š/ forms predominate in Natashquan, but I do not believe this is the case. The first two speakers are originally from Maliotenam, where the change has not taken place, even in the youngest speakers' speech, and Goose Bay, which also retains the /š/ uniformly. The 42-year old, the most remarkable informant I interviewed, has, as far as I was able to determine, /h/ in all places in his normal conversation, as is expected in the younger population at Natashquan, but for the purposes of the interview he carefully restored as many /š/'s as he was able, missing on only 7 items. He also restored vowels normally apocopated in the speech of all other informants, like the first /i/ in /nituːn/ 'my mouth', normally /ntuːn/. This command of at least two styles adumbrates the possibility that there are other speech styles in the repetoire of speakers of Montagnais awaiting investigation. I have used only one form of speech--word lists, and only one social situation--the interview, thus putting a limit on the efficacy of the findings. It is to be hoped that at some time in the future other forms of speech, like free conversation, traditional tales, song, as well as other social situations can furnish forth a much greater amount of data for further investigation of the generation gap.

## NOTES

[1] My field work in Montagnais was made possible by an Urgent Ethnology Grant from the National Museum of Man.

[2] I would like to thank David Pentland for advice concerning the Proto-Cree forms. I have not taken his advice in all cases, and any errors that occur are my own.

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# THE ARCHAEOLOGICAL IMPLICATIONS OF THE PROTO-ALGONQUIAN URHEIMAT

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This paper explores ways in which to interface historical linguistics and archaeology using the case of Proto-Algonquian in the context of Northeast prehistory. I use Frank Siebert's 1967 article on the Proto-Algonquian homeland as a starting point. By reexamining his analysis in light of more recent data and a somewhat more selective use of the available information, I have developed a geographic zone for Proto-Algonquian which appears to require some conceptual adjustments away from traditional forms by both linguists and archaeologists.

Siebert's initial analysis involved 53 plant and animal names reconstructed for Proto-Algonquian. Each was then mapped, and the zone of common occurrence was concluded to be the Proto-Algonquian homeland. There are difficulties with many of the names, however, and in my reexamination I eventually dropped all but twelve from consideration. I dropped all eighteen bird names. Names such as owl and hawk could not be identified as to species, and many others were both too difficult to identify specifically and too widespread to be useful for my purposes. The Pileated Woodpecker is easily identifiable, but is nevertheless too widespread to be useful.

I also dropped twelve of Siebert's nineteen mammal names in my analysis. Names like buck and fawn do not refer to single species. Fox and squirrel are names that can each apply to more than one species, which together are ubiquitous in North America. Beaver, muskrat and bear are unmistakable single species in the East, but also too widespread to be useful. I dropped the Harbor Seal because it is not terrestrial and therefore does not lend itself to the distributional format I have adopted. Nevertheless, the Harbor Seal can be useful, and is picked up again below after the completion of the distributional analysis of other species.

The elimination of a dozen mammals leaves us with raccoon, lynx, moose, porcupine, woodchuck, bison, and caribou as species that are not easily confused with other species and are at the same time not ubiquitous. However, there are hazzards with even these remaining six species. The ranges I have plotted for them are the maximum ranges known for the historical period. The raccoon has expanded northeastward in recent centuries, and we can guess that all mammal ranges have fluctuated similarly over the last few millennia. Thus, range boundaries must be regarded as only approximations (see Banfield 1974).

A further difficulty with animal species is the tendency of some to compete locally. This, of course, is rarely if ever the case with plant species. For example, the moose and caribou appear to be mutually exclusive in specific localities, even though their diets are quite different and the plants preferred by both might be available. Finally, direct archaeological evidence for animal species is rare and faunal samples usually biased in the Northeast.

I have dropped seven of the twelve tree names listed by Siebert. "Conifer" is too general to be of use. Names for ash and spruce are not specific, and several others are too widespread to be useful. The five names that remain are tamarack, elm, basswood, sugar maple, and beech. Each is easily identified by anyone with a knowledge of the forest of the East, and each has a relatively restricted range. Even better, the archaeologist has some direct access to prehistoric distributions of these arboreal species through palynology. The use of such species is not without its own hazards, for such things as bog growth and forest fire cycles can cause apparent distributional shifts, but it is less hazardous than the use of most animal species. Anthropologists interested in using such distributions should be careful to avoid the common pitfalls. For example, there is no necessary direct correlation between minor fluctuations in forest composition and similarly minor climatic variations over the last few thousand years. Similarly, anthropologists should avoid borrowing forest associations from out-of-date botanical sources, for these are basically intuitive constructs that have little to do with individual species distributions (see Snow 1972).

I dropped all four of Siebert's fish names on grounds that species are difficult to identify, the names are easily transferred to other species, and aboriginal distributions are difficult if not impossible to reconstruct with any accuracy.

By successively superimposing pairs of the remaining dozen distributions on a map of eastern North America, I was able to define an area within which the speakers of Proto-Algonquian probably lived. It was not simply a matter of progressively narrowing the area of overlap, because there was no overlap at all for one pair, bison and caribou. In that case, I concluded that the center of the Proto-Algonquian distribution must have been somewhere near enough to the distributions of bison and caribou such that its speakers could be familiar with both. As it happens, the information obtained by overlapping all possible pairs is nicely represented by the single pair beech-tamarack. The area defined includes the Great Lakes lowlands (apart from those of Lake Superior), the St. Lawrence lowlands, New England, and the Maritime Provinces. This is a much larger area than the section of southern Ontario defined by Siebert. We can reduce the area somewhat by reintroducing the distribution of the Harbor Seal and thereby eliminating the Great Lakes lowlands above Niagara Falls, but we are still left with a much larger area than that proposed by Siebert. My conclusion is that we cannot further reduce the area in our search for a Proto-Algonquian homeland. Rather, we must adapt historical linguistic and archaeological concepts to fit the limits imposed upon us by the data. If we cannot (or refuse to) do that, we will be unable to close the epistemological gap between what we wish to discover and what the data will allow us to discover.

In order to set the stage for further interpretation, I now turn to analogous data from Australia. There seems little doubt that the speakers of Proto-

Algonquian were hunter-gatherers. With appropriate allowances for the relatively higher natural productivity of the Northeast, we can usefully compare our case to historically known Australian hunter-gatherers (see Birdsel 1975:362). The first Australians reached their continent around 27,000 years ago, a date reasonably close to an arrival date I find acceptable for the first Americans. The contact population was about 300,000. This was composed of dialect tribes that ranged in size from about 200 to about 1500, the average size being about 500. Below 200 a group tended to fuse with another group. Groups that expanded to 1500 tended to split. The constituent units in all cases were patrilineal bands (hordes) of about 25 people. Simple arithmetic indicates a total of about 600 dialect tribes for the Australian continent at contact. Australia contains almost three-million square miles, or about ten square miles per person at contact. This implies an average of about 5000 square miles per dialect tribe, an area about the size of Connecticut.

The point of the Australian digression is to indicate some of the limits on our interpretation of the nature of Proto-Algonquian and its homeland. Even if the population were as thin in the area I have defined for Proto-Algonquian as it was in contact-period Australia (which it probably was not), there is room within it for two dozen or more speech communities. Whatis the meaning of our reconstructed Proto-Algonquian in the context of this apparent cultural-geographical reality? Resolution of that question requires another digression, this time into concepts of historical linguistics.

It is sometimes convenient to think of a set of related languages as having evolved as a consequence of successive splittings of a smaller number of earlier languages, all of them descending from a single parent proto-language. This presumes what I call a dendritic model, wherein it is further presumed that the proto-language had reality as an L-Simplex (Hockett 1958:323), a language made up of a set of mutually intelligible idiolects. It is a useful model, but utility by itself does not argue conclusively for the notion that a particular reconstructed proto-language was ever a literal historical reality as an L-Simplex. Indeed, if we adopt a population model rather than a dendritic model, as students of biological evolution have done, we come to a very different conclusion regarding the nature of proto-languages. In this model, a reconstructed proto-language is best thought of as a normative construct. We may have reconstructed some components of what looks like a real language, but those components may well stand for the relatively vague and diffuse linguistic condition Hockett calls an L-Complex (Hockett 1958:324). To make the point as forcefully as possible, it seems to me that the reconstructions or extinct forms by both linguists and archaeologists are often filled with the same misconceptions as an attempt to reconstruct the bodies of Adam and Eve through an exhaustive biological comparison of living human populations.

My main point is that while Proto-Algonquian might have been a real language spoken by a few hundred people in one relatively small part of the beech-tamarack area, there is no compelling reason to assume that was the case. It is equally likely that Proto-Algonquian is a normative construct that stands for speakers of related dialects that might have been scattered throughout the area and perhaps even beyond the border I have defined. The best we can do at this point is say that Proto-Algonquian was probably spoken within the beech-tamarack area, a statement that leaves a considerable amount of both linguistic and geographic latitude.

Having dismissed what I regard as spurious accuracy in historical linguistics, I do the same for archaeology. Phases, traditions, and other units of archaeological discourse have also tended to take on a spurious reality. Yet they too are normative abstractions. Moreover, archaeologists do not customarily think in terms of population models, and the systems approach I think must be adopted by archaeologists is far from maturity. Finally, the criteria for the archaeological definition of cultural continuity and discontinuity in both time and space have not yet been satisfactorily codified. Nevertheless, so long as a population model is kept in mind along with an appreciation for the limits of our data and concepts we fit them into, some preliminary interfacing of linguistic prehistory and archaeology in the Northeast is possible.

Mary Haas' best guess for a split between Algonquian and Gulf languages is 5000-6000 years ago (Haas 1958:258). Although he now disowns the data as being too recent, Siebert (1967:39) gives a time depth of 3200 years for Proto-Algonquian. Haas (1958-258) gives us a slightly more recent date for Proto-Central-Algonquian. There is some archaeological continuity in the beech-tamarack area between the dates suggested by Haas and Siebert. We should not assume that all the bands in the area necessarily have Algonquian descendants, nor should we insist that Proto-Algonquian was necessarily a real language spoken over either part or all of the area. I suggest that what we have sometimes called the Boreal Archaic (Byers 1959), including Ritchie's Laurentian tradition and Tucks Maritime tradition may well be the heart of the archaeological entity (Ritchie 1965:79-103, Tuck 1971:350). We may well be able to extend the generalization to include Old Cooper (see Griffin 1961). I realize that some archaeologists see this archaeological entity as the ancient foundation for later Iroquoian cultures, but I see other origins for these cultures and I predict that my association of it with Proto-Algonquian will be borne out with time. We can hope for more precision in the future, but I would caution that such precision might be an illusion. I end with an urging that we be prepared to cope realistically with the normative nature of our abstractions.

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# L'ACCENTUATION DANS LE MONTAGNAIS DE LA MOISIE

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Le dialecte du montagnais dont il sera question ici regroupe les communautés indiennes de Sept-Iles, Maliotenam et Schefferville, à l'exception des Naskapis, anciennement de la région de Fort Mackenzie. Si nous avons choisi de baptiser ainsi ce dialecte c'est parce que la plupart des locuteurs avait auparavant leurs territories de chasse et de pêche dans la vallée de la rivière Moisie ou dans les territoires avoisinants. Les justifications linguistiques de l'isolement de ce parler sont nombreuses. Face au dialecte de la Côte Nord qui le délimite à l'est, de celui de Bersimis au sud, et du Naskapi avec lequel il fait frontière au nord il présente plusieurs particuliarités qui servent à lui fournir un caractère relativement unique. Il s'agit d'un dialecte mixte en [n/l] mais avec un emploi prédominant du [n] s'opposant ainsi au dialectes du sud. Par contre il s'oppose aux dialectes de l'est par l'absence de voyelles [i], a brèves, réduites partout à [ə]. Les différences par rapport au Naskapi sont surtout d'ordre lexicale ou morpho-syntaxique, la négation en apu + conj. par exemple mais sur le plan phonétique on peut signaler un conservatisme dans l'emploi du [š] face au Naskapi qui semble l'avoir réduit partout à [h]. Il se peut aussi que les phénomènes d'accentuation que nous commentons ici servent à marquer ce dialecte par rapport à ses voisins mais n'ayant pas pu comparer en détail nous nous abstenons pour l'instant de toute généralisation.

Par accentuation nous entendons le processus ce par quoi une syllabe est mise en relief par rapport à d'autres du même mot, syntagme ou phrase. Dans les langues du monde ce processus peut être effectué de plusieurs façons différentes.

(i) par un changement d'intensité
(ii) par la durée relative
(iii) par le ton relatif

(iv) par un changement de timbre

(v) par une combinaison de plusieurs de ces phénomènes

De façon générale on peut affirmer que pour le montagnais de la Moisie l'accentuation est effectuée par une augmentation de la durée bien que dans au moins un cas que nous avons répèré cette augmentation de durée est accompagnée d'un changement de timbre et dans d'autres cas nous soupçonnons un changement de ton sans toutefois être capable pour l'instant de le démontrer.

TABLEAU I

| 2 syllabes | | 3 syllabes | | 4 syllabes et plus | |
|---|---|---|---|---|---|
| [məš'pʊn] | "il neige" | [uta'pan] | "dans l'auto" | ['astəmətət] | "le mur" |
| [nə'pi] | "l'eau" | [u'tapan] | "l'auto" | [məsəmə'teʊš] | "le sac" |
| ['nutn] | "il vente" | [wa'siu] | "il est jeune" | [n'tʌkwʊšən] | "j'arrive" |
| ['wašat] | "Sept-Iles" | [paste'u] | "ils sont secs" | [məšənigə'nask$^{w}$] | "crayon" |
| ['tsimən] | "allumette" | ['ušəgi] | "une peau" | [u'tejmənən] | "une fraise" |
| [mi'jus] | "boîte" | [tekwʊ'šən] | "il arrive" | [tʌkʊšə'nu] | "il arrive" |
| ['neu] | "quatre" | ['pasəgən] | "fusil" | [katšə'pikənis] | "une piàce" |

A première vue la distribution de l'accent dans le mot montagnais semble presque aléatoire. Un coup d'oeil sur un échantillon de mots tel que présenté dans le tableau 1 révèle que l'accent est susceptible de figurer dans à peu près n'importe laquelle position à l'intérieur du mot. En même temps un locuteur natif détecte facilement le déplacement de l'accent que caractérise inévitablement le parler d'un étranger et corrige sans difficulté une mauvaise prononciation en restituant l'accent à sa place habituelle. Il advient donc que parmi les connaissances linguistiques d'un montagnais il faut inclure sa capacité de connaître la place de l'accent à l'intérieur de chaque mot.

Evidemment il est possible que ce type de connaissance ne soit que purement lexical, c'est à dire qu'au moment d'acquérir chaque vocable le sujet apprend la position de l'accent. Dans ce qui suit nous n'avons pas écarté cette hypothèse mais en même temps nous avons voulu tenir compte du fait que pour un très grand nombre de mots la position de l'accent peut varier et ceci presque de façon systématique ce qui nous fait penser qu'au moins certaines positions de l'accent à l'intérieur du mot soient déterminées par des régles. A titre d'exemple (1) sur votre feuille..

| | | | |
|---|---|---|---|
| 1. | (a) | [pas'teu] | "il (inan) est sec" |
| | | [paste'u] | "ils (inan) sont secs" |
| | (b) | [n'tʌkušən] | "j'arrive" |
| | | [tʌkwʊsə'nu] | "il arrive" |

Encouragés par l'existence de ces régularités nous nous sommes livrés à une analyse détaillée de la distribution de l'accent ce qui nous a permis d'établir un certain nombre de généralisations lesquelles sans toutefois expliquer toutes les données constituent un premier pas vers la démystification de ce phénomène.

On peut formaliser les résultats de cette première analyse de la façon suivante.

Règle 1 Dans un mot qui ne contient pas de voyelle longue l'accent tombe souvent sur la dernière voyelle s'il s'agit d'un bisyllabe

exemples 2 [məš'pʊn]
[ə'sət]
[mə'sən]

Mais il existe des exceptions

exemple 3 ['məsn̩]

Pour les mots contenant plus de deux syllabes toutes brèves il est difficile de faire des généralisations puisque nous trouvons l'accent à peu près partout.

exemple 4 [n'tʌkwʊsən]
[nəmə'səstn]
[tʌkwʊ'sən]

Règle 2 Dans un mot qui ne contient qu'une seule voyelle longue l'accent tombre sur cette voyelle.

exemples 5 ['tsimən]
[pə'tetət]
['pasəgən]
['astəmətət]
[məsəmə'teʊs]

avec quelques exceptions

examples 6 [u'šət] son pied
[tekwʊ'šan] il arrive (forme changée)

Règle 3 Dans un mot qui contient deux voyelles longues ou plus l'accent tombe sur la dernière longue si le mot finit en syllabe brève.

exemples 7 [na'peu]
[sa'kigən]
[pne'šišət]

si le mot finit en syllabe longue l'accent tombe sur la penultième longue

exemples 8 ['uškat]
['šišip]
['šipu]
[u'tapan]
[a'pjutan]

[u'tejmənən]

[u'takwʊsit]

[ntej'migʷʊdi]

avec trois types d'exceptions

(i) les bisyllabes exemples 9 [a'pu]
[mi'juš]

(ii) les L B L exemples 10 [kasə'nu]
[asə'ni]

(iii) les mots de plus de deux syllabes qui contient trois longues ou plus
exemples 11 [stapjuda'nu]

Les généralisations que nous venons d'énoncer ne sont guère satisfaisantes. Elles servent à décrire des données jusqu'à un certain point mais ne fournissent aucun indice d'explication tout en laissant certains cas notamment des mots en LL, LLL, et LBL sans généralisation du tout.

Face à cette impasse un examen plus minutieux des données phonétique s'est imposé. En effet l'analyse de sonogrammes a dévoilé l'existence de trois degrés de longueurs de voyelles relativement bien définies. Le tableau II fournit une classification des voyelles de surface en termes de longueur.

TABLEAU II

| breves 6.0 - 8.5 | longues 10 - 16.0 | allongées 17.5 - 26.0 |
|---|---|---|
| [nəpi] | [i nəpi] | [ī nəpī] |
| [kʌp] | [e utenau] | [ē škwadēm] |
| [sʊkʷ] | [a mətaš] | [ā mətaš] |
| | [u šipu] | [ū šipū] |
| | [ʊ̄ kʊp] | |
| | [ə̄ məsə̄n] | |
| | [e̯u se̯ukʷ] | |

L'aspect intéressant de ce tableau c'est que les voyelles semblent se diviser en deux groupes dont le trait distinctif n'est pas la longueur absolue de la voyelle mais sa longueur par rapport à une autre voyelle possédant un timbre généralement identique. Toute voyelle aurait donc une variante allongée.

On peut illustrer ces faits à partir d'une comparaison des formes du singulier et du pluriel de noms inanimés où l'accentuation constitue dans la plupart des cas la seule marque distinctive.

exemples 12

| Nom | Singulier | Pluriel |
|---|---|---|
| terre | [si] | [sī] |
| porte | [škwadem] | [škwadēm] |
| bas | [mətaš] | [mətaš] |
| rivière | [šipu] | [šipū] |
| soulier | [məsn] | [məsə̄n] |
| robe | [kʌp] | [kʊ̄p] |
| chaudron | [sʊ$k^w$] | [sə͜u$k^w$] |

Dans chaque cas la formation du pluriel est le résultat d'un allongement de la dernière voyelle pas du tout en termes absolus mais par rapport à la forme du singulier. Il semble clair donc qu'il existe en montagnais, du moins dans ce dialecte deux types de longueur. L'un qui est sous-jacent ou si l'un préfère lexical l'autre qui semble être le résultat de processus réguliers capables d'être décrits sous forme de régles. Le tableau III illustre la façon dont nous proposons d'en tenir compte.

TABLEAU III

| | ə | ʌ | ʊ | i | e | a | u | ʊ̄ | ə̄ | eu | Ī | ē | ā | ū | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| HAUT<br>ʊ | - | - | + | + | - | - | + | + | - | - | + | - | - | + | | | |
| POST. | - | + | + | - | - | - | + | + | - | + | - | - | - | + | | | |
| BAS | - | - | - | - | - | + | - | - | - | - | - | - | - | - | | | |
| LONG | - | - | - | + | + | - | + | - | - | - | + | + | + | + | | | |
| ACCENT | - | - | - | - | - | - | - | + | + | + | + | + | + | + | | | |

Le fait que ces voyelles peuvent être regroupées en deux séries nous permet de suggérer qu'en montagnais les voyelles sous-jacentes ne sont en fait qu'au nombre de sept et que ces voyelles sont modifiées par une régle d'accentuation qui a comme effet d'augmenter la longueur de la voyelle dans certaines circonstances. Nous postulons donc un trait [plus accent] qui aurait comme effet de doubler approximativement la longueur de la voyelle à laquelle il se rattache. Les voyelles de surface pour le montagnais peuvent donc être définies selon le tableau III tandis que les voyelles sous-jacentes ne seraient que les sept premières voyelles de ce tableau.

Il nous reste à formuler les régles de l'attribution du trait [plus accent]. D'abord d'après cette hypothèse il parait que l'accent n'est pas du tout lexical en montagnais. Toute racine capable de figurer isolément ne porte jamais d'accent. Les brefs sont toujours brefs et les longs ne sont jamais allongés.

On ne retrouve pas les voyelles (13) [ʊ], [ə], [eu], [Ī], [ē], [ā], [ū] dans des mots invariables. Les quelques exceptions peuvent être expliquées par des processus d'accentuation dont il ne sera pas question ici à savoir l'accentuation "émotionnelle"

exemples 14 [e'ukʷun] exclamation de surprise

[mãwat] non catégorique

ou contrastive

exemples 15 ['nĪn nəmitšədi mawat tšin] "Moi j'ai mangé mais pas toi"

Déjà notre hypothèse permet d'expliquer certaines des généralisations que nous avons formulées sur une base intuitive à partir des données présentées dans le tableau I.

D'abord il est évident que comme l'accent est manifesté en montagnais à travers un allongement de la voyelle il est naturel de vouloir situer cet accent sur la seule syllabe longue dans un mot qui, à part celle-ci ne contient que des brèves. Mais comme nous suggérons il ne s'agit pas d'un accent du tout. S'il s'agissait d'un accent on s'attendrait à ce que les voyelles antérieures dans (16) [tšimən] ou [pətetət] soient respectivement [Ī] et [ē] mais comme témoignent nos sonogrammes ceci n'est nullement le cas.

De la même façon notre hypothèse explique pourquoi des mots comme 17 [tši'mən] ou [utejmə'nan] constituent d'apparentes exceptions à nos généralisations c'est à dire sont des mots où l'accent ne tombe pas sur l'unique longue ou la pénultième dans le cas de mots qui en contiennent deux. Il s'agit ici d'un véritable processus d'accentuation qui obéit à la régle que nous postulons pour la formation du pluriel inanimé à savoir accentuation de la dernière syllabe.

Finalement nous expliquons la distribution aléatoire de l'accent dans des mots de types LL, LBL, LLL etc. Dans ces mots à moins qu'il ne s'agisse d'un autre processus d'accentuation, l'accent contrastif par exemple ou débit de phrase, nous ne nous attendons pas à ce qu'il y ait de véritable accent. On explique ainsi pourquoi si souvent l'on à des transcriptions contradictoires pour des mots comme (18) "rivière"

par exemple ['šipu] [ši'pu] "rivière"

ou (19)

['ušəgi] [ušə'gi] "peau"

Si dans ces deux cas la première variante est plus fréquente c'est certainement parce qu'on a voulu insister sur la différence par rapport au pluriel.

Il nous reste à préciser la forme des régles d'accentuation. Ce que malheureusement nous ne sommes pas en mesure de faire de façon définitive sans pousser plus loin nos recherches. A moins de possèder les intuitions d'un locuteur natif du montagnais la seule façon de vérifier les distinctions fines implicites dans les oppositions i/ī, e/ē etc. reste la vérification par sonogramme.

Nous pouvons quand même démontrer que l'utilisation de l'accent ne se limite pas à la formation du pluriel inanimé.

Seulement l'accentuation peut expliquer des données pour le pluriel animés illustrés dans le tableau IV.

TABLEAU IV

| | | | |
|---|---|---|---|
| [ponešiš] | oiseau | [pənešišət] | oiseaux |
| [kawtšu] | pneu | [kawtšūt] | pneus |
| [tuan] | balle | [tuān] | balles |
| [tapəškagən] | foulard | [tapəškagə̄n] | foulards |

Evidemment le pluriel est habituellement formé en ajoutant le morphème /-t/ à des noms finissant en voyelle ou la variante [-ət] après consonne.

exemple 20 [ušəki] [ušəkīt] "peau" (animaux)

[minuš] [minušət] "chat"

mais le sonogramme revèle qu'en plus de la suffixation il y a un allongement de la voyelle précédente. Quand il s'agit d'une voyelle longue comme dans nos deux exemples la voyelle créée est doublement longue ce qui présuppose l'introduction du trait [plus accent].

Ceci devient particulièrement évident quand il s'agit de noms animés finissants en n (21) [tuan] "balle" ou [upastəman] "avion". Ces mots perdent leur morphème de pluralité sous l'influence de deux régles phonologiques qui ont comme effet d'éliminer respectivement le [ə] en postion [n-t] et [t] en position n-#. La première de ces régles est généralisée et explique les groupements consonnantiques dans des mots comme (22) [tšisentəman] "si je savais" [tentʊm] "il pense" etc. ainsi que le [n] variante du préfixe première personne que l'on retrouve dans (23) [ntuan] "ma balle" [ntəšənəkašən] "je m'appelle". La deuxième régle est nécéssaire pour expliquer la troisième personne singulier du présent du conjonctif des verbes (TA) de la première conjugaison. Là où on s'attendrait à trouver (24) [tʌkwʊšənt] on ne trouve que (25) [tʌkwʊ'šən] ou (26) [tekwʊ'šən] à la forme changée. La même régle explique des formes comme (27) [mitšwakān] "sur la table" ou [tetəpwa'gən] "sur la chaise" où la désinence locative est absente en surface.

On doit donc supposer pour [tuān] "les balles" une dérivation comme dans (28)

| | |
|---|---|
| sing. | t u a n |
| pluriel | t u ā n ə t |
| chute de ə | t u ā n ∅ t |
| chute de t | t u ā n ∅ |

Ces régles expliquent aussi bien ce cas et d'autres semblables où le nom animé possède une dernière voyelle longue que les formes comme (29) [uǎpwagən] "les pipes" [wapekigə̄n] "des savons" où la voyelle finale de la racine est brève. Les mêmes régles s'appliquent dans la dérivation de ces dernières formes.

Evidemment on pourrait prétendre que ce sont des exceptions ou même qu'il y ait eu confusion de genre mais on note que des noms comme (30) tuan uspwakən continuent à prendre la forme animée du verbe transitif

| | | |
|---|---|---|
| exemple 31 | [nwapmaw tuan] | je vois la balle |
| | [tǎwapmaw uǎpwakən] | tu vois la pipe |

La preuve tranchante du fait qu'il y ait eu usure de la désinence pluriele se trouve dans la forme interrogative ce ces mots où le [t] ne tombe pas

| | | |
|---|---|---|
| exemple 32 | [tuānta] | "Les balles?" |
| | [uǎpwagə̄nta] | "Les pipes?" |

Jusqu'à maintenant nous avons signalé deux cas importantes où l'accent semble actif dans la morphologie du montagnais de la Moisie mais peut-être serait-il déjà evident à partir de certains de nos autres exemples que l'utilisation de l'accentuation ne se limite pas à ces phénomènes. Par exemple le locatif des noms finissant en [n] se distingue du non-locatif par l'accentuation de la dernière syllabe

| | | |
|---|---|---|
| exemple 33 | [utapan] | l'auto |
| | [utapān] | dans l'auto |
| | [pasəgən] | le fusil |
| | [pasəgə̄n] | dans le fusil |

Un autre cas concerne les formes comme [tʌkwʊšə̄n] "il arrive" (3 pers conj.) qui s'oppose à (35) [ntʌkwʊšən] j'arrive (1 pers ind.) qui se prononce souvent (36) [tʌkwʊšən].

Un autre cas oppose les troisièmes personnes singulier et pluriel du présent de l'indépendant de verbes animé-intransitifs dont la racine termine en voyelle longue.

exemples 37 [wasiu] il est jeune
[wasiū (t)] ils sont jeunes

la parenthèse ici indique que le (t) final est parfois absent.

En comparant ces divers cas on remarque que pour tous sauf le inanimé il y a de bonnes raisons de postuler une chute de voyelle associée à chaque cas d'accentuation. Les morphèmes de pluriel nominal animé, locatif, pluriel verbal 3 pers et 3 pers sing. conjonctif ont tous une variante [-ət]. On remarque que quand cette marque est absente et qu'il s'agit soit de (t) soit de rien la voyelle précédente est accentuée. Il semble donc avoir une motivation forte pour reconstruire une voyelle sous-jacente (ə) dand le cas du pluriel inanimé. De cette façon les cas d'accentuation que nous venons de signaler s'expliquent tous par une seule régle phonologique.

exemple 38 V (n) ə (t) # → $\overline{V}$ (n) (t) #

Cette régle est suffisament générale pour permettre d'expliquer encore des paires minimales séparées par la seule présence de l'accent. Il s'agit cette fois du singulier et du pluriel 3ième personne du présent de l'indépendant des verbes inanimés intransitifs finissant en voyelle.

exemple 39 [pasteu] il (inan) est sec
[pasteū] ils (inan) sont secs

Pour expliquer cette forme il suffit de postuler un [ə] sous-jacente comme marque du pluriel pour les verbes (II).

Il va sans le dire que tout ceci coincide avec ce qu'on peut reconstruire historiquement comme proto-montagnais. Les parlers de la Côte Nord ainsi que le Naskapi et les parlers de Baie James témoignent tous d'une voyelle toujours présente en surface là où nous avons voulu postuler un [ə] sous-jacente.

Il reste à savoir si l'accentuation est identique dans ces parlers, ceci nous fournirait une aide précieuse dans l'éclaircissement d'autres aspects de l'accentuation que nous nous trouvons pas à expliquer avec la régle (38).*

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# SOME CONSIDERATIONS IN DEVELOPING AN ORTHOGRAPHY FOR THE MICMAC LANGUAGE

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Berry (1958) has given a discussion of a number of considerations bearing on alphabet design. This paper[1] will analyze the application of these considerations and others (cf. Rubin and Jernudd 1971:278) to the current situation in the Micmac language, and will address some problems in orthographical design. I will also show how the determination of the best orthographical representation for specific words or morphemes can have broader implications for the overall spelling system of a language. We will also examine certain linguistic principles of orthography suggested by Halle (1969), Klima (1972), and others, and show their utility and limitations. It will be suggested that only the most transparent phonological alternations in a language should count toward eliminating symbols in an optimal orthography. Nevertheless, practical considerations can override some of these theoretical considerations, and may lead to a better orthography for the situation in hand.

Considerations in orthographical design are of several sorts. One major function of a spelling system is to promote native literacy. For example, Navajo literacy (CAL:10) 'for adults ... seemed desirable [because] (a) it would provide access to practical information...; (b) it would help Navajos to identify with their own culture.... Such identification would greatly help Navajos to adjust to modern situations...; (c) Adult literacy would facilitate parental involvement in the education of their children; (d) Adult literacy would also be of great use in the everyday affairs of the Navajo community [eg] ... communication between speakers of Navajo....' Cf. also Bowers (1968:394-395) and Kaye (1975).

A theoretical distinction can be made between the overall design of an orthography (how many symbols to use; which symbols to use to represent what, etc.) and the specific spelling of individual morphemes or words. These considerations are, however, closely interconnected. Levi (1975:3f) reports

Don DeBlois's resumé of reasons for having a widely usable Micmac orthography: to have and publish a permanent record of old people's stories, legends, etc.; to publish articles, newspapers, books, etc. in Micmac; to publish dictionaries and grammars of Micmac; to facilitate teaching Micmac and teaching of other subjects in Micmac; teaching the scientific method in schools. A good orthography, according to DeBlois, should be based on a thorough investigation of Micmac, should take dialect differences into consideration, and should, so far as is possible, be consistent; it should be easy to use in reading, writing, spelling, typing, and printing; it must be understood, accepted, and believed in by fluent speakers of Micmac, and parents whose children might learn it. Kaye (1975) discusses 'pseudo-orthographies', which are used basically only as aids in assimilating children into the dominant society. I assume that this is not anyone's purpose in devising a Micmac orthography, and mention it no further.

It is generally agreed among those who have considered the question, that 'sub-phonemic' variation--that which is predictable--should not be indicated differently in an optimal orthography. Since obstruent voicing, for example, is nearly predictable in Micmac, an orthography such as Rand's which does indicate voiced obstruents differently from the voiceless ones is to that extent nonoptimal. Certainly, in current discussion by Micmacs and linguists, the starting point is most often Pacifique's orthography, which--unlike Rand's--never uses two symbols to indicate this type of subphonemic phonetic variation. The complaints one can make about Pacifique's orthography are rather in the other direction--not enough distinctions are made, some persons have claimed, to have the most efficient and useful orthography. We discuss this further below.

It should be made clear that this is quite a different thing from the claim made by Berry (1958:739): 'There is general agreement that phonetic ambiguity is bad and that words pronounced differently should be kept graphically distinct. It is also agreed that the orthography should be such that one who knows the pronunciation of a word should be able to spell it.' This claim, as it stands, is quite wrong. Consider the unstressed, reduced vowels of English. For example, it might appear that the last syllable of the words in (1a)[2] should be spelled the same, since each is pronounced [-fɚ]; but the related words in (1b) show that under stress, the different orthographic vowels indeed represent different

| | | | | | |
|---|---|---|---|---|---|
| (1) | (a) | súlfur | [sʌlfɚ] | sulfúric | [sʌlfyurɪk] |
| | | cámphor | [kæmfɚ] | camphóric | [kæmfɔ́rɪk] |
| | | kéfir | [kɛ́fɚ] | keffíric | [kɛfɪrɪk] |
| | | pedálfer | [pədælfɚ] | pèdalféric | [pɛdælfɛrɪk] |
| | | Núphar | [nufɚ] | (alternant pronunciation:) | [núfàr] |

phonetic vowels; it is only in unstressed position that they are pronounced identically. In a language like English, with extensive vowel reduction, a semiphonetic script would, first of all, tend to obscure relationships between different shapes of the same morpheme, as in (1) above; secondly, we would require a symbol for shwa, which is never phonemic in English--at worst it must be arbitrarily assigned to a spelling if no other forms exist with stress on that syllable. (There is some evidence--see Fidelholtz 1975:206--that all nonalternating shwas in English are assigned to the phoneme [ʌ] by speakers.) This example supports Halle's proposed principle (1973:19):

(2) Orthographies must contain no symbols that reflect the operation of phonological rules.

We will later see reasons, however, to modify (2).

A similar but less familiar example from English involves vowel nasalization. Vowels occurring before nasal consonants in English are always nasalized. What is not usually realized is that when the nasal consonant occurs before a voiceless consonant, it is not normally pronounced in most varieties of American English. Thus we find

(3) (a) can [kæ̃n]
(b) canned [kæ̃nd]
(c) can't [kæ̃t] (rarely, or in largo speech: [kæ̃nt]).

In fact, as example (3c) makes clear, the /n/ is morphophonemically so obviously present that it is very difficult to convince speakers that it is not present phonetically. No one has ever suggested using a different symbol for nasalized vowels in English orthography (or even in English phonemics), since this is so clearly against the intuitive, morphological, and distributional evidence--it would double the number of spelled vowel symbols, while at the same time obscuring morphological relationships.

The Navajo conference (CAL:4-5): '...agreed that the orthography should reflect the phonemic structure of Navajo on the basis of one simple or complex symbol for each sound unit. It was realized that in questions of spelling there would be problems, especially in morphophonemic matters. It was suggested that when possible a uniform spelling be maintained where pronunciation changes occurred as a result of morphophonemic processes. It was suggested that as the orthography was used a careful record be kept of problem areas and that psycholinguistic tests be made to determine whether change to a more phonetic spelling made reading easier or not .... such testing would ... be carried out through the careful work of well-trained, competent specialists observing a variety of subjects ranging in ability, age, and other ... criteria.' Cf. also Haas (1969:6), Sapir (1963) and Smalley (1964a:7). Note also Venezky (1970a:261): '...relating single letters to more than one sound ... will in general depend upon the morphophonemics of the language. We would not, for example, consider representing /d/ and /p/ in English by the same letter. Where frequently occurring morphophonemic alternations are involved, however, there is a potential use for bi-valued forms.' Voorhoeve (1964b:131) writes only the basic (and not the phonetic) tonal forms for Trique to simplify word recognition in reading, which he takes to be 'the identifying of words and word groups, and only thereafter the pronunciation of the words aloud.'

It is instructive here to examine the experience in orthographies of related languages. (See Kaye 1975 for more discussion and proposals for various Algonquian languages.) Severn Ojibwa uses the Cree syllabic system, with a few additions. <"> represents [h], but is omitted (Todd 1972:358). Todd says (ibid.) '... the absence of <"> may produce occasional ambiguity .... Contrasts between preaspirated or fortis stops and simple lenis stops in the same position in word pairs is at any rate very rare in Ojibwa .... Syllabic writing practices suggest that this convention, while helpful to the learner and important to linguists, is not so critical to native speakers.' Likewise, the Cree symbol for vowel length, '... a dot placed above the appropriate syllabic, ... is omitted in all Ojibwa writing examined. This is in keeping with the general lack of contrast between long and short vowels .... The principal contrasts occur between [various verbal suffixes]. In these cases as well vowel length is omitted ..., even though a symbol is available. Clearly there is little need for a native speaker to represent vowel length, and such an indicator might well be optionally omitted in actual use of an alphabetic system.'

(ibid.:358f). Manny Metallic (personal communication) has noted independently what Todd suggests: 'The use of macron does not add to the length of the written word, and can be deleted with minimal distortion of the appearance of the word.' (Compare our discussion of Micmac shwa below.) She continues, 'For these reasons the use of the macron in a standardized Ojibwa orthography can be recommended.' Based on experience with developing programs for native literacy, she notes (p. 359) that '... the use of consonant and vowel symbols familiar to linguists has met with general approval. The principal reservations concern the use of diacritics, because these symbols are not available on a standard English typewriter keyboard. Rather than adopt digraphs such as ... <oo>, which would lengthen the written words, it seems reasonable to propose the use of diacritics when feasible, and their omission otherwise. Ojibwa syllabic writing demonstrates that a less than perfect phonemic writing system is adequate for a language in which minimal pairs of any kind are extremely rare.' John Nichols, in a paper at this conference gives several examples from Potawatomi traditional writing of benign underdifferentiation (see also Nichols 1975). Frantz (1973:27) appears puzzled by the failure of a native person to indicate the presence of a distinctive glottal stop. The above discussion indicates that this might be generally expected if the distinction is a relatively marginal one in Blackfoot. Cf. also Smalley (1964a:10): 'If ... distinctions which are ignored or confused [do not] carry an important functional load ... underdifferentiation may not be at all serious.' He makes a similar point in (1964b:38ff). Nida (1964a:26-27) points out that all orthographic systems regularly underdifferentiate phonemic intonational distinctions with no great loss. Smalley (1964e:143) suggests for Gipende a new orthography which differentiates between plain and aspirated phonemes only if their functional load is high. Similarly, Gudschinsky (ibid.) notes, 'Not all ... successful orthographies have been maximally efficient.'

Klima (1972:59) observes that with respect to the orthography, even an 'ideal language-knower' may not have equal access to all levels of language structure. It then becomes a strategic question 'which levels of linguistic structure ... are ... most readily accessible in the process of reading and writing' and 'for which levels ... we have the strongest linguistic intuitions...' that is, whether the orthography should reflect the underlying form or the phonetic [ie, phonemic] form for words. If the orthography reflects underlying forms--ie with little or no predictable phonetic variation indicated--then the identity of common morphemic elements is orthographically preserved. If, on the other hand, one uses a relatively 'phonemic' orthography then pronunciations are readily available from the written form of words, with little or no application of phonological (or graphological) rules. Klima gives various possible principles to relate orthographies to underlying forms and phonological (from the point of view, however, of one who knows the language). While all reasonable alphabetic systems maximize economy (e.g. no suggested Micmac orthography uses a separate symbol for the [b] and [p] sounds, which are predictable), there are a number of degrees of abstractness represented by existing orthographies. English corresponds to Klima's principle IIIa, which presupposes the dominance of underlying sound segments (as O'Neil 1972:112 points out though, only insofar as the morpheme in question is regularly affected by the phonological rules). This is a modification of Halle's principle (2) above. O'Neil (1972:112f) also points out that Faroese and other languages as well as English seem to indicate that Klima's principle IIIa is a very natural one for orthographic systems. Chomsky (1970:12, 13, 16) and, indirectly, Venezky (1970b) also support this principle. (But see the discussion of Frantz 1973 below and ibid. 9-11 for some suggested constraints on the use of this principle.)

See Nida (1964a:25) for an application of this principle.

It is important to note, however (cf. Venezky 1970a) that the evidence on what principles are best for an orthography are mostly anecdotal and intuitive. Gibson and Levin (1975:284f), for example, criticize various versions of a stimulus-response decoding view of the reading process. And Chomsky (1970:15) even questions the relevance of phonological studies to the teaching of reading. Similarly, Nida (1964b:150f) advises phonemic rather than morphophonemic spellings where the (quite regular) alternations are opaque.

The Navajo orthography (CAL 1969:3f) was intended primarily for the needs of Navajo schools, ... especially for publications for these schools ... it was agreed that uniformity in such matters as the use of symbols, capitalization, etc. should be maintained in published materials ... no rigid uniformity was ... advocated for private work or for handwriting. The conference ... was very firm in its stand for flexibility in such matters as shapes of letters, patterns of spelling and punctuation, so long as preferences in such things did not conflict with the general uniformity of published texts.

... the native speaker of Navajo [would be] the principal user of the writing system ... there would also be non-Navajo speakers using the writing system but ... their needs ... would differ from those of native speakers since non-Navajo speakers would obviously need more guides to pronunciation than native speakers.

O'Neil (1972), referring to Read's (1971) study of children's spontaneous development of writing systems, notes that '... the child's phonology seems to be quite separate from his morphology, whereas the orthography reflecting adult phonology relates them in nearly optimal ways. And this difference may in fact be where the difficulties (insofar as these are linguistic) in children's learning to read may lie. For if standard orthography presupposes phonological perceptions that children have not yet acquired, then it is not going to be easy for them to associate standard spellings with the right things in their perception.

The above discussion implies that, insofar as evidence is available, orthographies for adult native speakers are most natural and suitable insofar as they do not indicate regular or productive phonological alternations. Irregular, minor, or opaque alternations, on the other hand (such as strong verb ablaut in English, or verb contraction in Micmac) are best indicated in the orthography, rather than by using their underlying forms. (Cf. Frantz 1973:28: constraints I-IV, which he suggests to limit the abstractness of orthographies along the lines suggested by Kiparsky 1971 for phonology. Pulgram earlier suggested a parallel between phonology and orthographic systems.) Compare the discussion in Kaye (1975).

Pre-school children, on the other hand, seem to operate on a much more concrete, phonetic basis than adults do. (Read 1971 gives the example of a spontaneous spelling chrie for try--the /t/ is palatalized before /r/, but entirely predictably in English). O'Neil (1972:115n) points out some indirect evidence that children have acquired adult perceptions of writing and reading by the third grade. It might then be suggested that children either be taught in a much more superficial orthography than adults, and later changed over (but note that such experiments as the Initial Teaching Alphabet have not had remarkable long-term success); or that teaching of the reading process be deferred until the children have acquired adult phonological perceptions.

Sociocultural considerations generally also favor this latter method, and, for example, the Navajo conference 'in general agreed that all reading for Navajo-speaking children should be started in the mother tongue.' (CAL 1969:9).

Native speaker illiterates would most probably fall in the first category, in which the 'morphologically-based' system would be preferable. Non-native speakers, on the other hand, especially those who are illiterate in any language, are likely to be helped by a more concrete orthography, but for different reasons than the children they have (presumably) adult perceptions, but they lack the native speaker's facility with the morphological alternations of the language, and need more 'pronunciation guides', as it were (e.g., in Micmac, diacritics to indicate predictable shwas, cluster voicing, etc.). As they become more fluent in the language, these 'guides' can (and will) be dispensed with. Cf. Gudschinsky (1969:15).

Contrary to what one might think, however, a 'morphological' orthography (i.e. one based on Klima's principle IIIa) is no hindrance to previously-literate second language learners (e.g. nonfluent Micmacs literate in English, learning Micmac). Lotz (1972:122) says, 'Although English pronunciation does create some difficulties, it is very easy for foreigners to learn to read and spell English. So it is not the intricacy of the spelling but rather some kind of initiation in the writing-reading processes which seems to pose the relevant problem.'

## SOCIOLINGUISTIC CONSIDERATIONS

In designing (or redesigning) the orthography, one must lose sight of neither the history of Micmac orthographies (especially the recent history), nor of the current contact situation of Micmac, and other languages which Micmacs can be expected to know how to speak. The number of Micmacs is currently estimated as between 8,000 and about 15,000 individuals, of whom perhaps half or more are fluent speakers of some variety of Micmac. Nearly all Micmac speakers are bilingual in English--a very few older persons (as well as a few pre-school children) are Micmac monolinguals, and some people know French as well as English and Micmac, but ther are now virtually no Micmac-French bilinguals, so far as I know. Historically, the Micmacs were first in contact with French speakers, and many words of modern Micmac were borrowed very early from French (e.g. *magasan* 'store', *laqalans* 'barn', *tēsipow* 'horse', from respectively *magasin*, *la grange*, and *des chevaux*). During most of the last two centuries, however, most contact of the Micmacs has been with English speakers (even of the Gaspesian Micmacs, very few indeed currently even know French).

There are also a variety of historical Micmac orthographies. One of the earliest is an ideographic system apparently devised by LeClercq several hundred years ago, revised by Maillard, and in which a catechism was published about 100 years ago by Kauder, and reissued more recently by Pacifique. (Hoffman 1955:265ff). Virtually no one remains able to read these, much less write in them. In the last half of the 19th century, the Rev. Silas T. Rand, who apparently knew the language fairly well, developed an orthography derived from English, with some double vowel symbols, use of both members of the pairs <b/p,d/t,g/k>, and other poor design features (in terms of modern phonemic criteria), compounded by a somewhat inconsistent application of some of his (largely implicit) principles. Earlier, he had used a phonetic orthography developed by Pitman. Much of the Bible was translated by Rand, and he compiled the most widely available dictionary of Micmac. Nevertheless, very few or no persons currently write, and few read in Rand's orthography. (Williams and Jerome 1975, however, do use doubled vowels for long vowels--see below.) Another orthographical tradition for Micmac, however, traces back to the early French missionaries, and was apparently first begun, or at least continued, by Maillard. In this century, Pacifique, who was apparently quite fluent in Micmac, modified Maillard's orthography and published catechisms, a grammar, a newspaper, etc. in this orthography--the one which most of that relatively small number of (mostly older) Micmacs who are currently literate in Micmac read and write in. Just as the English-speaking Rand's orthography was largely based on English orthographic principles, likewise Pacifique's orthography, developed by French-speaking priests, shows the influence of French orthography principles, although perhaps more indirectly (e.g. his falure to use <u> has been attributed to the [ü] value of the grapheme in French). Pacifique published relatively large editions of his several Micmac words, which are the published materials which most literate Micmacs have access to. This by itself would be an argument for adopting or adapting Pacifique's orthography (compare CAL 1969:1). Note that the conference on Navajo orthography finally adopted an orthography for Navajo which was essentially a slightly modified version of a system in which a considerable amount of literature already existed (CAL 1969:3). Gudschinsky's introduction (1969:14) is worth quoting here:

> 'Unfortunately, the problem of orthography is one of the most explosive in the world. Differences about alphabets have frequently caused shooting wars, riots, and serious political division. In some languages, competing groups using different orthographies have actually perpetrated large competing literatures. In the light of all this, it seems important that major [emphasis added--JLF] revision of an orthography be undertaken only if there are severe problems with its readability, or if social opposition makes it unusable.'
> Cf. also Nida (1964a:23): 'It is not what is easiest to learn, but what people want to learn and use which ultimately determines orthographies.' Cf. Nida (1964a:29, 30), Smalley (1964c), and Garvin (1954:118ff).

We should, however, keep in perspective that in Micmac, most materials in Pacifique's orthography are of a religious nature, and only a relatively small number of persons are literate in it. Nevertheless, it is at least a familiar orthography to most Micmacs, and changes in it should, it seems to me, be made with great caution. Ray (1963:75-6) also points up the uncertainties in predicting reaction to suggested changes.

As Joshua Fishman (1975 Washington Linguistics Club lecture) has pointed out, there is no necessary connection between knowing a planned aspect of a language (e.g. an orthography), liking it, and using it. People can know it, but neither like nor use it; like it, but neither know nor use it; know and use it but not like it; etc. He points out that in general older people are better at liking (or disliking) an orthography (e.g.) while younger people are 'better' at knowing, and especially using, the newly planned entity. That is, it is naturally easier to accept a new orthography (say) if that is the only one you have ever been exposed to--tradition not only has a very deep and pervasive effect on people, but it also can develop surprisingly rapidly. Cf. also Fishman (1972:55-56), Trager (1972:306-310), Rubin and Jernudd (1971:101, 205, 213), Ferguson (1968a:34), and Bowers (1968:396) for a discussion of a wide variety of sociolinguistic difficulties in instituting orthographic reform. Bowers (ibid.) suggests that one situation in which the case for revision is strong is 'where there is more than one alphabet in current use in one language.'

Gudschinsky (1969:14f) emphasizes the readability of an orthography, which can be tested by having people read it; there is no abstract theoretical principle that will predict readability.

'To be used, however, an orthography must also be socially acceptable. If the people reject the orthography of their own language, [its] potential readability...will not help it ....'

'She (ibid.) says, 'Of course an orthography should be practical in terms of typewriters and linotypes. This should not be a primary consideration, however. The readability of an orthography is far more important than the cost of adding a key to a typewriter or linotype.' Smalley (1964a:14) makes the same point and (1964b) criticizes the reliance of Moore orthographers too heavily on typographical convenience.

Since evidence is mounting (cf. Fidelholtz 1975:207-208, Hsieh 1970) that speakers internalize something like the 'phonemic' shape of all words--whatever else about them they may internalize--it is important that linguists retain critical objectivity on the issue of whether a morphophonemic orthography is superior to a phonemic one. Before we can make informed decisions on this issue, the astounding and profound abyss in experimental studies on this issue must begin to be filled in. Until then, we must use our best-informed judgment, historical evidence, and whatever informal experiments with native speakers that we can muster. A good rule of thumb is to be very cautions of morphophonemic alternations which can become obscured phonetically by other alternations--in such opaque cases it may be better to rely on phonemic rather than morphophonemic principles (cf. O'Neil 1972:112). Gudschinsky (1969:16) points out that morphemes which change phonologically in different environments pose the problem of whether 'to spell them always with the same basic spelling.... There should be testing to discover which way of writing is easier to read. There is no general rule which fits all languages.'

The above considerations show that, as Gudschinsky (1969:14f) points out, a purely phonemic orthography 'with one symbol for each phoneme ... is seldom practical ...,' even though such an orthography is widely assumed to be ideal. Cf. Taylor 1974:T5; 'One of the best writing systems is one which reflects speech with great accuracy....' Otherwise, 'the reader or writer must use his memory more: he must learn spelling rules and remember to apply them.' Taylor really is criticizing only those rules 'based on something outside the language' (e.g., in his case, English orthographic principles). In Taylor's Lakhota system, spelling is seldom in doubt, because the native speaker 'writes largely as he speaks.' 'Each symbol represents only one sound.... There are no silent letters.' (Ibid.:T6). Cf. also Smalley (1964a:7).

Gudschinsky notes (ibid.), 'Where all or most of the speakers of a language must learn a second language, transition value becomes important. Does a given orthography help or hinder the reader in making the transition to a second language?... '...Probably the greatest transition value comes from simply learning what reading is, and to expect different spellings to equate with different pronunciations. Cf. Grimes et. al. (1964:115) Smalley (1964b:35ff, 44f), and Walker (1969:1ff). Taylor (1974:T6(2)), Armstrong (1968:234), Bowers (1968:392-393) and Ferguson (1968b:259) discussion of the relationship of an orthography to that of the national language in a wide variety of situations.) The Navajo conference (CAL 1969:4) decided that 'the question of transfer to and from English orthography ... was one significant element ... but ... it was considered much more important to base the orthography on the facts of the Navajo language itself....'

Some similar considerations result from dialectal or conversational variants. Thus Jones (1948:3f) gives the following examples from English: 'When ... people pronounce *sure* as if it was written *shaw*, the peculiarity passes almost unnoticed, ... but it would be clearly inadvisable to admit *shaw* as an alternative spelling of *sure*. It is likewise doubtful if it would be expedient to indicate in writing the 'strong' and 'weak' forms of such words as *can*, *have*, *from*, *was*, *at*, using different spellings of [e.g.] ... *can* in *I will if I can* and *You can see for yourself* ... as the use of strong and weak forms does not as a rule involve any difference of meaning it is unquestionably better that the words should always appear in the same written forms in an orthography for current use. In the case of the word *at* it would not be possible to show the weak form in writing without introducing an extra letter; our present alphabet cannot distinguish between the two sounds of *at* in *What are you looking at?* and *I'm looking at the moon*. It has to be observed, too, that the use of weak forms differs with different people and with the style of speaking.'

## MICMAC: GENERAL CONSIDERATIONS

At a meeting of Micmacs and linguists concerned with developing a usable and uniform Micmac spelling system held in Truro, N.S., 15 to 17 September 1975, many issues were discussed and a great deal of headway made. Another meeting in late November in Fredericton, N.B. produced further discussion of various issues, but no agreement on a uniform orthography. (See Levi 1975). In these meetings, certain tentative decisions were made. Part of my purpose is to comment on those decisions and to recommend others.

Let us consider the sound system of Micmac and how it has been proposed to represent this sound system in an orthography. All suggested orthographies are alphabetic, and to a greater or lesser extent based on Pacifique's orthography. Using linguistic symbols, the sound system of Micmac can be represented as:

(4)

| Consonants | | Vowels | |
|---|---|---|---|
| Obstruents | Sonorants | Short | Long |
| p s t č k q | m n l w y | a e i o u ə | ā ē ī ō ū |

Pacifique's orthography (Pacifique 1939) is, respectively:

$$(5)\quad \mid p\ \ s\ \ t\ \ tj\ \ g\ \ \hat{g} \mid m\ \ n\ \ l\ \ o\ \ i \mid a\ \ c\ \ i\ \ \hat{o}\ \ o\ \ (ə) \mid \begin{Bmatrix}\bar{a} & \bar{e} & \bar{\imath} & \bar{\hat{o}} & \bar{o}\\ a & e & i & \hat{o} & o\end{Bmatrix}\mid$$

Rand's correspondences to (4) (Rand 1888:ii and passim) are (less frequently used variants are parenthesized):

$$(6)\quad \mid \begin{Bmatrix}p\\ b\end{Bmatrix}\ s\ \begin{Bmatrix}t & ch & k & k\\ d & j & g & g\\ & & & hk\end{Bmatrix}\mid m\ \ n\ \ l\begin{Bmatrix}w & y\\ (oo) & i\end{Bmatrix}\mid \begin{Bmatrix}a & & \\ \hat{a} & \bar{a} & e\\ \breve{o} & \breve{e} & \breve{\imath}\\ \breve{a} & & \end{Bmatrix}\ o\begin{Bmatrix}oo\\ \breve{o}\breve{o}\end{Bmatrix}\ \breve{u}\mid \begin{Bmatrix}a & & \\ \hat{a} & \bar{a} & e\\ (\breve{o}) & (\breve{a}) & (i)\\ (\breve{a}) & & \end{Bmatrix}\ o\begin{Bmatrix}oo\\ (\breve{o}\breve{o})\end{Bmatrix}\mid$$

Francis and Hale (1970) suggest two systems, the first closely following Pacifique's:

(7) | p s t tj g ĝ | m n l ö i | a e i ô o ë | ā ē ī ô̄ ō |

and the second for typing (suggested in the Appendix):

(8) | p s t tj g q | m n l w i | a e i o u v | ah eh ih oh uh |
(or k) (or delete)

Metallic and Delisle (1975)'s system, which again closely parallels Pacifique's, is:

(9) { p s t j g ĝ / pp ss tt jj gg ĝĝ } | m n l w y | a e i o u ê | à è ì ò ù |

Williams and Jerome (1975)'s system (mainly based on Pacifique's system) is, respectively:

(10) | p s t j g g | m n l w i | a e i o u ' | aa ee ii oo uu |

Rand's orthography (6) seems at least partly to fall in the class of those discussed by Nida (1964:133), which 'involve so heavy a communication load that they tend to fatigue the reader unduly; thus ultimately ... discouraging widespread literacy ....' Accordingly, it is not a serious contender for consideratio as a national Micmac orthography.

Other suggested systems include:

| | | | |
|---|---|---|---|
| (11) | c | for | č |
| | x | for | q |
| | V: or V· | for | V̄ |
| | V̀, V́, V̄, or VV | for | V̄ (all to be used). |

A few other sounds in Micmac have been ignored by those suggesting alphabets (correctly so, in my view): e.g., [š, h, r] in a few borrowings, interjections, and sporadically elsewhere; [? or x] in a few predictable environments. Some of these will be discussed below.

The <v> to represent shwa was apparently abandoned, in favor of either <ê> (or <ė>) or <'> (see below). In light of Walker's (1969) discussion of attempts to develop a writing system for Passamaquoddy, a closely related language to Micmac, this appears to have been a wise decision. In a first, 'Unsuccessful writing system, ... [n]ative speakers of all ages showed considerable interest ... but, in general, failed to become literate .... Some objected to the use of <v> to represent a vowel ...' (Walker, 1969:164). Some of the objections raised below against using <ê> or <ė> for shwa apply to <v> as well.

The <x> suggested for [q] has apparently been abandoned, largely because of the negative transfer with its normal English value of [ks] or [gz], as well as its unfamiliarity.

Placing a colon or a raised dot after a vowel to indicate length has been objected to on two main grounds--it increases the length of the word, and has been construed as aesthetically unpleasant. The latter objection would also apply to a preceding (as opposed to raised) hyphen. Note also Manny Metallic's comments above on the use of a macron to indicate vowel length.

At the September meeting, a tentative decision was made to drop <c> for [č] in favor of <j> (or <tj>). Two factors might prompt this decision--negative transfer from English values for <c> (none of which are [č]); and, more importantly, that this is closer to Pacifique's orthography and would both make transfer easier, and prompt less resistance from those speakers already literate than <c> would.

It was also accepted tentatively by the participants that <w> would be used after the symbols for velar and uvulars (<k> and <q>, or <g> and <ĝ>) to indicate the labiovelar (or labiouvular): <kw, qw> or <gw, ĝw>, at least in word-final position. Some who accepted this, however, are resistant to using the symbol <w> in other positions--intervocalically or postvocalically. Again, this seems quite reasonable, even though the labialization (i.e. the [w]) is often predictable--either from a preceding [ū],or [w], or from a following underlying /u/.

Most of the suggestions are somewhat different from suggestions for other Algonquian languages (cf. Jones and Todd 1971 and Kaye 1975, e.g.). Since the other languages are not intelligible to Micmacs without special study in any case, however, this does not seem to be a crucial consideration.

Williams and Jerome (1975 and personal communication) and Metallic and Delisle (1976) have noticed several types of alternation in the pronunciation of sequences of sonorant consonant (m, n, l) followed by an obstruent consonant (p, t, g, s, j, q). There are basically two types of pronunciations of such consonant clusters: symbolizing the sonorant consonants by R, the voiceless obstruents by T, and the voiced obstruents by D,[2] we find only the following pronunciations: [-R?T-] (12a) and [-R̄D-] or [-R̄D-] (12b) (actually, the phonetic realization of -R?- depends on the sonorant: /m?/ and /n?/ are pronounced [m?] and [n?], respectively, but /l?/ normally becomes [ł] (voiceless [l]); /w?/ becomes [w?], [wx], or [wk], depending on dialect and context):

| | | | | |
|---|---|---|---|---|
| (12) (a) | men?p'g | 'I bit it (an.) off' | len?tug | 'deer' |
| | n?gat | 'my foot' | n?jugwijij | 'my mother-in-law' |
| | win?sit | 'he's wicked' | m?sigu | 'grass' |
| | m?tijin | 'thumb' | m?g'sn | 'shoe' |
| | m?jegas | 'I will be dirty' | wełtesgul | 'I'm glad to meet you' |
| | tełgil | 'I'm that size' | ugumułjin | 'eight' |
| | wełs'tm | 'I hear it with pleasure' | | |

| | | | |
|---|---|---|---|
| (b) mn̄pās | 'Ill bit it off' | mn̄tu | 'devil' |
| mn̄gēn | 'remove it!' | tml̄gn̄jl̄j | 'hatchet' |
| mn̄si | 'untie me!' | pm̄pegittew | 'it'll flow' |
| tm̄tmu | 'oyster' | tm̄sēn | 'cut it!' |
| l̄pātuj | 'boy' | l̄tātaq | 'they will go' |
| etl̄sign̄tasit | 'he's being baptised' | l̄gusuaqan | 'ladder' |
| al̄jat | 'he staggers' | | |

There are predictable variations between the types of pronunciation in (12a) and (12b) when the cluster is preceded by another consonant, we find only the (b) pronunciation:

| | | | | |
|---|---|---|---|---|
| (13) | teɬkil | 'I'm that size' | t̄lgiɬtes | 'I'll be that size' |
| | tem?saqam | 'I partition it off' | tm̄zaqās | 'I'll partition it off' |
| | men?tm | 'I bit it off' | mn̄d'tes | 'I'll bite it off' |
| | tml̄gn | 'axe' | tml̄gn̄jl̄j | 'hatchet' |
| | (cf. mulin | 'mill' | mulin?čl̄j | 'sewing machine' and |
| | len?tuk | 'deer' | mn̄du | 'devil') |

Note that this holds true even if the preceding consonant is itself a sonorant, as in mntu. In word-initial position, clusters are of the (a) variety, unless the sonorant is contracted from eRc- of if the sonorant is /l/ (contracted from elC-: apparently there are no verbs beginning with leC-), in which case they are of the (b) variety:

(14) n?kat 'my foot', m?k'sn 'shoe'; BUT: l̄gusuaqan 'ladder', n̄tu 'lose it!'

After a vowel (i.e., elsewhere), the clusters are normally of the (a) type. From what has been said so far, it appears that this type of alternation is predicatble, so it would be unnecessary to indicate either the voicing distinction or the sonorant length in a practical orthography. Thus we would spell the words in e.g. (13):

| | | |
|---|---|---|
| (15) | telgil | tlgiltes |
| | temsaqam | tmsaqās |
| | mentm | mnt'tes |
| | mulinjīj | tmīgnjīj |
| | lentug | mntu, |

respectively. Persons who know the language will pronounce the words correctly because they have internalized the generalization stated above.

Naturally, there are exceptions to the above generalization. By far the largest group of exceptions is where we find cluster voicing after a vowel:

(16) gūntew 'stone' un̄ji 'head'
mal̄tew 'blood' (cf. maltəjjuei 'hammer') tel̄tāsi 'the way I think'
(cf. teltāsi 'the way I was beaten')
tel̄gīg 'it is that sharp' (cf. telgīg 'it is that big')

It appears that we would do well to have an orthographical means of indicating this distinction, since it is not obviously predictable. (Note, though, that Pacifique's orthography does not distinguish these types, with minimal ill effects for native speakers.) Since the type in (16) seems to be the irregular one, it is sensible to complicate the orthography, if necessary, in those cases rather than in the regular types like mntu, lentug, etc. Fidelholtz (1968--sporadically) and Francis and Hale (1970--consistently) use the device of indicating the length over the sonorant before the irregularly-voiced stops:

(17) gun̄tew, mal̄tew, un̄ji, etc.

Metallic and Delisle (1975) indicate the regular type by lengthening the obstruent, e.g. <lenttuk>. In 'The Micmac orthography' (1976), they have changed to always indicating the voiced clusters by means of an apostrophe <'> before the voiced obstruent: <gun'tew>, etc. Williams and Jerome (1975) use the same device, as well as using the apostrophe for other purposes. For reasons detailed below, I prefer to retain the <'> for use as a marker for shwa (which Williams and Jerome use it for as well). But we can indicate [gun̄dew] either as <gun̄tew> (as in Fidelholtz 1968 or Francis and Hale), or, perhaps even better, <gunntew>--note that the long sonorant and voiced obstruent occur also after another sonorant (mntu), so the latter orthographic device is readily interpretable from basic Micmac orthographic principles. Note also that Micmac has no phonetic sequences of identical consonants, but rather shows a long consonant where we would otherwise expect two identical consonants:

(18) [mulin̄] < /mulin +n/ 'mills',

so there will be no tendency to pronounce <gunntew> as *[kunəndew]. Students who are learning Micmac will need to be taught this orthographic principle, but should have no trouble with it than, say, English speakers learning German have in learning to devoice word-final orthographic voiced obstruents (e.g. <Bund>--->[bunt]).

A much smaller class of exceptions is like

(19) enn'm?tēm 'I nail it in'

This is irregular in that a consonant preceding the cluster does not cause voicing. Here, we can opt for the device used in Metallic and Delisle (1975) and later abandoned, spelling (19) as (20):

(20) <enn'mttēm>.

Alternatively, we would use some additional orthographic symbol, such as <.>, to indicate an irregular pronunciation:

(21) <enn'm.tēm>

The former method seems preferable here, unless it would tend to obscure morphological relationships. There is some evidence (Williams, personal communication) that (19) was historically [en̄am?tēm], with a vowel before the cluster. If this is correct, then the 'historical' spelling would make the word orthographically regular, if the vowel reduction could be predicted by readers learning the orthography.

Words like [n̄du] 'lose it!' from *entu* [en?tu], as opposed to *nepay/npa*! [n?pa] 'sleep!' show that the former must be orthographically indicated; even if it can be predicted from the underlying verb before contraction, afterwards there is no distinction: <npa>/<ntu>. But if we indicate this with a double <n>: <nntu>, we will be distorting the relationship to other forms of the verb more than if we use the macron: <n̄tu>. Note that if we decide to use the macron in such cases: <n̄tu>, <gun̄tew>, that does not preclude our writing double <n>'s elsewhere, e.g.: <nastesinn> 'you're caught' < /nastesin+n/.

The above generalizations about obstruent voicing in clusters with sonorants have further consequences. Verbs with an /e/ in the initial syllable usually lose this vowel in certain tenses (e.g. the future), or in combination with prefixes, a process called contraction, thus: pemiey 'I'm walking', pmiās 'I'll walk'. Note the words

(22) [kewxči] 'I'm getting cold' [kewxsəm] 'I saw it down',

but the contracted forms

(23) [kūjites] 'I'll get cold' [kūzətes] 'I'll saw it down'

This is evidence that these words contain an underlying /w/; since /w/ is a sonorant consonant we would get

(24) /kewči/ -----> [kewxči] (/w?/ -----> [wx]),

but

(25) /kwčites/ -----> /kw̄jites/ -----> [kūjides].

Especially in the latter case, we see a benefit in not having to spell it <gūji->, since this word is exactly analogous to /tlkil-/ -----> [tl̄gil-]: the difference, however, is that while Micmac does not distinguish phonemically between long and short /l/, there is a phonemic distinction between long and short /u/. That is, spelling [kūjites] as <gwjites> would allow the fluent Micmac reader to predict the long, syllabic /w/ (i.e. [ū]/, just as he can predict the long syllabic /l/ in <tlgiltes> -----> [tl̄giłtes]. At the same time, the regular morphological relationship with *gewji* would not be obscured. Now notice that this observation, to be put into effect, requires that we have an orthographic symbol for /w/ different from that for /u/; otherwise, the fact that [kūjites] has a long vowel would have to be indicated orthographically--interconsonantal [u]'s may be long or short:

(26) *gūgūgwes* 'owl' VS. *gugwej* 'you're it (playing tag)'.

Also, if we do not recognize that there is a rule governing the insertion of a glottal stop (or velar fricative in predictable cases), then we will be faced with a multitude of problems, in addition to difficulties in conveniently representing such alternations as [kewči-]/[kūji-] above. First of all, in many environment, e.g. after /w/, the phonetic character of the /?/ varies from [x] to [k] to [?], depending on the particular Micmac dialect, style, and context. Secondly, we would presumably have to represent this element somehow (an apostrophe <'> has been suggested to represent the *absence* of /?/ in consonant clusters, i.e. voicing of the following consonant), which means representing orthographically an element whose distribution is effectively limited to occurence in a predictable environment: between preceding sonorant and following obstruent after a vowel.

There are other facts supporting this analysis. For example, the pejorative suffix in Micmas is -*j* (*npign* 'my hand', *npignj* 'my dirty or disfigured hand'). But the pejorative form of *wāw* 'egg' is *wāwxj* 'rotten, etc. egg'.

Contraction provides even stronger evidence for <w>. Verbs beginning with *weR*- behave normally: *wenaqayey* 'I jump', *wnaqayās* 'I'll jump'. But other verbs beginning in we- behave somewhat abnormally:

| | | | | |
|---|---|---|---|---|
| (27) | wegāy | 'I'm mad' | uggwāytes | 'I'll be mad' |
| | wejaqamiey | 'I'm boiling' | ux$^w$jaqamiās | 'I'll be boiling' |
| | wesōtm | 'I save it' | ux$^w$sōttes | 'I'll save it' |
| | wetapey | 'I get punished' | ux$^w$tapās | 'I'll get punished'. |

But if we assume that these latter futures are all regular:

(28) wgāytes
wjaqamiās
wsōttes
wtapās,

then, since the sonorant obstruent clusters are not in the lengthening/voicing environment (after another consonant), we expect them to have /?/ epenthesized, giving us

(29) /w?gāytes/
/w?jaqamiās/
/w?sōttes/
/w?tapās/,

which end up pronounced like (27) above (the [k] in the first gets labialized by the preceding /w/--see Fidelholtz 1968, 1976).

Such regularities argue that we should spell contracted forms without the underlying /e/; another reason is that a few irregular verb stems with initial-syllable /e/ fail to undergo contraction, and would make reading the contracted stems quite difficult. Voorhoeve (1964a:65) notes that spelling underlying Sranan vowels which are phonetically elided gives 'a very unnatural effect when reading ... even if ... not reading aloud...'

There are few clear instances arguing for <y> like those with [w] above. One apparent case--forms of [ēym] 'be (there)'--can be otherwise handled (see Fidelholtz 1968:89ff, 98ff; Gilles Delisle [personal communication] has also argued against <y> in ēim). But see the examples in (39b) and (40) below.

Shwa in Micmac is also very largely predictable. As Metalic and Delisle (1976:2-3) point out, shwa is morphophonemically predictable in many instances. For example, whenever an obstruent consonant (p, t, j, g, q, s) is followed by a sonorant consonant (m, n, l), the sonorant is predictably syllabic--that is, pronounced so that it acts like a vowel. This is true word-initially as well as elsewhere, so that in plamu 'salmon', for example, the l is syllabic. Likewise:

(30) tmīgn 'axe'   glu 'eagle'   gmniēuti [g'm̩ni-] 'communion'

While there is not necessarily a shwa pronounced in all such instances of syllabic sonorant consonants (e.g. in gwitn 'canoe' there is no vowel whatsoever between the t and the syllabic n), and therefore an analysis of all syllabic consonants as consisting of a shwa plus the consonant is somewhat forced; nevertheless this analysis has been maintained, e.g. by Metallic and Delisle. Since nearly all such sequences in Micmac contain syllabic sonorants, however, we would like to avoid indicating them in the orthography if possible.

Another environment where sonorants become syllabic is between consonants or before an obstruent word-initially:

(33) pgu [əpku] 'gum' tplutaqan [ətplutaqan] 'law'
sqolj [əsqolč] 'frog' gjigan [əkčigan] 'city'

Since no such words fail to have a shwa inserted there is no reason to indicate it orthographically in these instances either. Similarly, in clusters of three consonants a shwa regularly appears between the second and third consonant:

(34) nsttes [nsət:es] 'I'll understand him' < /nest+tes/
gtgālās [əktəgālās] 'I'll make him drunk' < /getg+āl+ās/

Nevertheless, there are a number of cases like (35) with no shwa intercalated:

(35) psgwesewās [əpskwezewās] 'I'll reap'
wsgās [ukʷskās] 'I'll fish',

which might lead us to wish to indicate such multi-consonant cluster shwas even if they are predictable, for reading ease.

But since most shwas are predictable, it would seem ill-advised to use an alphabetic or diacriticized symbol for shwa. Rather, a relatively inconspicuous simple diacritic appears to be the best solution. This suggestion gains plausibility when we realize that persons learning Micmac who are not yet fluent speakers (and this will be a substantial portion of even Micmac children becoming literate in Micmac) will need as many pronunciation aids as possible. On the other hand, it seems unnecessary to burden the orthography for fluent speakers with predictable shwas, for example, although, as we see below, some are not predictable and will have to be somehow indicated in any case. But if we use the diacritic symbol <'> to indicate shwa, then when Micmac learners (whether children, adults, or whites) become fluent enough to be able to predict shwas even where they are not orthographically represented, the predictable <'> can be dropped with a minimum change in the appearance of the word. Thus the words of (34) would be spelled

(36) ns'ttes    gt'gālās    ws'gewōqtuas

in standard Micmac, and

(37) 'ns'ttes    'gt'gālās    'ws'gewōqtuas

for beginners. The arguments for writing shwas in *some* places where they are predictable to make reading easier is quite similar to Metallic and Delisle's (1976:12) reasons for writing <w> and <y> even though they are always predictable. See also Kaye (1975:23f).

This also points the way for exceptional words whose shwas cannot be predicted. For example, from *epteg* 'it's hot' we would expect the future **pttew* [əptədew]. Instead, we irregularly get *p't'tew* [pədədew]. Similarly, *gepsaqātu* 'I close it (a book)' has the irregular future *g'p'saqātutes*, rather than *[əkpəza-]. These shwas must be orthographically indicated, and perhaps for shwas which are completely unpredictable a case can be made for using an alphabetic symbol such as <ê>: <gêpêsaqātutes>. But for the normal predictable shwas discussed above, either no symbol should be used, or the diacritic <'> for learners. This way the orthographical difference between a word with shwas indicated: <m'ntu> and one without predictable shwas: <mntu> will be minimal, and not a significant problem in transference as reading facility is increased.

Another argument for the 'optional diacritic' method of indication for shwa is seen in words like /angitēlmg/, which can be pronounced [an?kidēləmk], [an?kidēlmək] or [an?kidēləmək]. Note the differences in the representations of the three pronunciations, using the diacritic (38a), and using <ȇ> (38b):

| | (a) | (b) |
|---|---|---|
| (38) | angitelm'g | angitelmèg |
| | angitel'mg | angitelêmg |
| | angitel'm'g | angitelêmêg |
| neutral: | angitelmg | (?no neutral representation). |

Observe that the (38b) forms appear quite different, although the actual pronunciations are rather similar, while the (38a) forms appear very similar. Also, with the (38b) forms, one is virtually forced to pick one or another of the possibilities, while with the (38a) type representation, one can leave all the shwas out, and the result does not appear significantly different.

Many of Metallic and Delisle's (1976) arguments for <ê> instead of <'> or nothing are specious rather than sound. They claim (p. 2) a 'morphophonemic relationship between e and ê, but their examples (2) show no consistent relationship: teluey/tèlua is not congruent with nepay/ènpa or (p. 3) pegisin/êpgisintes. Likewise, their claim (p. 21) of parallel syllable structure for pasteg and pastèg is a little misleading. One might make the same comments about the y and i of the English words randy and sandhi; but the syllabication of English orthographic <y> is entirely predictable, and <y> serves as a marker of other phenomena. In Micmac, shwa-insertion in triconsonantal clusters is all but wholly predictable.

However appealing some of their arguments about shwa, the basic one relating it to /e/ is clearly specious; other more valid arguments for using shwas in the orthography do not necessarily support their use of <ê>; in fact <'> for shwa seems eminently more defensible, with the possible exception of unpredictable shwas discussed above. This is rather similar to the situation in Hebrew or Arabic orthography, wherein the vowels may or may not be represented, with little loss in intelligibility *because vowels in Semitic languages are hardly at all relevant to morphemic meaning* (although they do change with tense, number, etc.). Even more so, in Micmac shwas are of almost no importance in distinguishing morphemes, or even number, tense, etc. Thus, representing them by orthographic 'throwaway' symbols is quite sensible.

The term 'throwaway' is chosen advisedly. It must be faced by anyone proposing an orthography with diacritics that very often the diacritics will fall by the wayside in writing, typing, or typesetting. Cf. Berry (1968:744-745). As MacCarthy (1969:91) says: '... the ... main disadvantages [of accents] are calligraphic and typographical: any handwritten stroke that has to be added separately, that does not form part of a letter, is a nuisance and either wastes time or tends to get left out--even the dotting of *i*'s and the crossing of *t*'s, so traditional that they are not looked upon as 'accents' are troublesome here....' Marck (1970:319) adds, 'i-dots are apt to float in outer space and jumbo lines to cross the several t's (as well as all other ascending characters) of a particular word.' Cf. also Gudschinsky (1967:4). 'Diacritical marks are difficult to teach. Digraphs are usually a more practical symbol, from a teaching standpoint, than extensive use of diacritical marks.' Note also that diacritics over letters can be problematical for capital letters in typing or typesetting. Thus, if the base symbol and the (symbol + diacritic) are significantly different in the phonology of the language, confusion can often result. So one using <e> for [e] and <ê> or <ė> for shwa, say, must expect the segments [e] and shwa to be confused sometimes in the orthography. Some evidence from Pacifique's orthography is relevant here. Pacifique used the symbol <o> for [u], [ū], and [w], and the symbol <o> for [o] and [ō]; he used the symbol <g> for [k], and <ĝ> for [q]; and he used <e> for [e], [ē], and [ə]. Very often, in the material he printed, the circumlfexes were omitted (even occasionally in his *Grammaire*). For [k] and [q], the resulting confusion is minimal, since [q] is nearly always predictable from /k/ after /a/ or /o/. Similarly, [w] is usually predictable from /u/. But confusing [o] with [u], or confusing [e] with [ə], can be problematical, especially for readers with minimal knowledge of Micmac, and should be avoided if at all possible. Thus, I would argue for separate symbols <o> and <u>, and significantly different symbols for [e] and [ə]. Keeping [k] and [q] apart is less crucial, but even here there are some minimal pairs (e.g. *magasan* 'store'/*maqamigew* 'earth') which may make it advisable to represent the two differently.

This line of reasoning poses problems when applied to the distinction between long and short vowels. The suggested method for indicating long vowels is with an acute accent: V́, grave accent: V̀ or macron: V̄ (the first and second being available on some Maritime bilingual typewriters as a dead key--lower and upper case, respectively--and the last on all typewriters as a raised hyphen (but requiring a backspace and a raising of the platen in typing). Long versus short vowels are certainly distinctive in Micmac (e.g. *nēpāl'g* 'I kill him' vs. *nĕpāl'g* 'I put him to sleep'), but minimal pairs such as the above are relatively rare, so that leaving off a macron is rather unlikely to affect understanding adversely. In addition, unlike the case for shwa, for [w], or for [q], there is no readily available symbol for the long vowels as opposed to the short vowels. The only other really feasible suggestion is a doubled vowel symbol for long vowels: <aa, ee, ii, oo, uu>, and this has been used by a few persons (cf. (10) above; Proulx and Piro have also used this system in some of their work.) But there is a great resistance to this on various grounds: historical (Pacifique's orthography never used it--although Rand's currently virtually unused orthography made some inconsistent use of this device); practical (words would be longer on the average, making typing and printing more expensive); aesthetic; and even some linguistic grounds (cf. Fidelholtz 1971; Metallic and Delisle (1976:4, fn. **) also note that 'short identical [vowel] segments only become long when there is no intervening word or morpheme boundary', and cite the examples ulāl [ulāl] 'be kind to him' from /wel+āl/ vs. ulaal [ula(?)al] 'these' from /ula+al/). Thus this possibility seems somewhat unlikely of acceptance, and the macron (or some other essentially diacritic device--<h> has also been suggested) seems to be the best suggestion from the totality of points of view.

Williams and Jerome's system (10) is quite ingenious in many respects--especially in its multiple yet consistent uses of the symbol <'>--for voicing, syllabic sonorants, and deleted consonants and vowels. Aside from the minor flaw in their system. The system consciously underdifferentiates (again, quite ingeniously), since, as pointed out above, many Micmac distinctions carry an extremely low functional load. But there are some irregular words, and they make no provision for indicating the pronunciation in these relatively rare cases, such as magasan and wetneyāsi. Literates will no doubt not find this a problem, but it will cause language learners severe problems.

Marck (1970) points out the marginal status of diacritics in English orthography, and mentions a variety of factors militating against the use of diacritics: technical ones--'the condition of 99.9% of the typewriters in use' (p. 324), 'typeface availability, compositorial practice' (p. 319) and 'more fundamental ones pertaining to linguistics, pedagogy,...' (ibid.). Similarly, one can criticize the choice of diacritics in Francis and Hale (1970), if not the use of diacritics altogether. At least in my dittoed copy, Francis' <pīgö> [pīk$^w$] 'night-hawk' looks very much like <pīgū>. It seems quite a reasonable constraint on orthographic diacritics that both a macron (¯) and a diaeresis (") should not be used distinctively (i.e., on the same 'base letter' to indicate distinctively different sounds), because of the great danger of their confusion, as in the above example.

Fidelholtz (1968) spends a great deal of effort in demonstrating the feasibility of deriving all occurences of phonetic [q] from an abstract /k/ after underlying /a/ or /o/; and likewise of deriving all occurences of phonetic [č] from an abstract /t/ before /i/. Insofar as this attempt is successful, however, it requires in some cases very abstract underlying forms, which are very distant from the actual phonetic shapes. This is especially true in the case of [č]. While Pacifique's orthography can be taken as indicating [č] and [q] by the addition of diacritics to <t> and <g>, respectively (namely <tj> and <g>) the major impetus, at least in the case of [q], for introducing the new symbol <q> # is to assure its distinctness from <k> # (or <g>). (A strong factor in suggesting both <j> and <q> is typographical convenience.) This seems to me to be perfectly reasonable, especially in light of the tremendous theoretical gynmastics necessary to push the [q] < /k/ and [č] < /t/ hypotheses through to their ultimate implication of eliminating /č/ and /q/ from the 'phonemic' inventory. This is not to suggest that no [č]'s, for example, come from /t/, but merely that perhaps not all do. The [t]/[č] opposition seems to me to be very similar to the [θ]/[ð] opposition in English--it was formerly completely predictable, but historical change has murked the picture so much that any modern predictability is marginal, notwithstanding the essential absence of minimal pairs.

Another very general rule of Micmac changes /t/ (especially the third person singular verb ending) into /k/ after a sonorant consonant (/l,m,n,y,w/). Thus:

| (39) | | Verb stem | | 3sg | |
|---|---|---|---|---|---|
| | (a) | eluēwie- | 'be crazy' | eluēwie-t | 'he's crazy' |
| | | jijjemā- | 'stink' | jijjemā-t | 'he stinks' |

| | | | | |
|---|---|---|---|---|
| | welaqapi- | 'be tipsy' | welaqapi-t | 'he's tipsy' |
| (b) | wegāy- | 'be mad' | wegāy-g | 'he's mad' |
| | telgil- | 'be that size' | telgil-g | 'he's that size' |
| | nastesin- | 'be caught' | nastesin-g | 'he's caught' |

After this rule has derived [k] after sonorant stems, however, some sonorants are regularly dropped:

| | | | | |
|---|---|---|---|---|
| (40) | megwēy- | 'be red' | megwē-g (< /megwēy-g/) | 'he's red' |
| | aqamīm- | 'snowshoe' | aqamī-g (< /aqamīm-g/) | 'he snowshoes' |

and if the vowel preceding the sonorant is /a/ or /o/, the /k/ then uvularizes:

| | | | |
|---|---|---|---|
| (41) | alām- | 'swim around' | alā-q (< /alā-g/ < /alām-g/ < /alām-t/<br>'he swims around' |

It is easy to see that, even though we can always predict the exact shape of the 3sg verb form if we know the underlying stem shape, it does not follow that we should always indicate the 3sg morpheme as <t>, for in words like those in (40) above, we could not tell the difference between, say, *welēg* and *wekāyg* and *jijjemāt* (presumably spelled, respectively, <welēyt>, <wekāyt> and <jijjemāt>). Similarly (41) would presumably be spelled <alāmt>, and reading problems could be predicted for this form. But even spelling *telgilg* as <telgilt> strikes me as inadvisable. Experimental evidence for these claims is badly needed (or at least native reactions) to support them or, conceivably, refute them.

Another example, discussed at length in Fidelholtz (1968:27ff) involves certain word-final vowel sequences. Long vowels are shortened in word-final position:

(42) /tapī/ 'bow' -----> [tapi] /tapī-g/ 'bows' -----> [tapīk]

/lnū/ 'Indian' -----> [lnu] /lnū-g/ 'Indians' -----> [lnūk$^w$]

However, there are no phonetic short (or long) [a] or [e] in word-final position in nouns. The arguments are quite involved, but the end results are /ā#/ -----> [aw] and /ē#/ -----> [ey] (see Fidelholtz 1968:32ff for details):

(43) /saqamā/ 'chief' -----> [saqamaw] (cf. /saqamā-q/ -----> [saqamaq] 'chiefs')

/wissē/ 'nest' -----> [wissey] (cf. /wissē-l/ -----> [wissē-l] 'nests')

This analysis is quite abstract, however, and even if it should turn out ultimately to be correct, it by no means follows that it should be reflected in the orthography. In fact, it seems most advisable to spell these words <saqamaw, saqamaq, wissey, wissēl>. Cf. the comments in Metallic and Delisle (1976:4, 12).

An example of predictable variation involves the word [newxt] 'one, alone'. From the discussion of gewji ([kewxči]) above, one might conclude that we should spell the word <newt>. However, in verbal combinations, this stem get contracted in the future, and becomes [n?kuti-], which must have come from an underlying /nekut(+i)/. This is subject to a metathesis rule, giving /neukt(+i)/. Nevertheless, it seems to me inadvisable to write the uncontracted form <negut>. Rather, it appears advisable to write the two forms as <newt(+i)> and <nkuti->.

The class of sonorant consonants (R) mentioned above ([m, n, l, w, y]) figures in another very general rule of Micmac. When a g or q is preceded by a long or short back vowel ([a, o, u]) and followed by a sonorant consonant, a 'short' copy of the vowel appears between the g or q and the sonorant:

(44) $-\left\{\begin{matrix}u\\o\\a\end{matrix}\right\}\left\{\begin{matrix}g\\q\end{matrix}\right\}R- \text{ -----> } -\left\{\begin{matrix}\bar{u}\\o\\\bar{a}\end{matrix}\right\}\left\{\begin{matrix}g\\q\end{matrix}\right\}\left\{\begin{matrix}u\\o\\a\end{matrix}\right\}R-.$

For example, the stem taq- 'hit' can occur with the 'transitive animate (object)' (TA) ending -m- or the 'transitive inanimate' (TI) ending -tm-. With the latter, we get e.g. taq-tm 'I hit it'. With the former, howe er, we get e.g. /taq-m-k/ 'I him him' -----> [taqamk]. An orthography could handle these facts in several ways. First, the 'copied' vowel could be written: e.g. <taqamg>. Secondly, since the quality of the copied vowel is predictable and it is short, it could be written as a shwa: <taq'mg> or <taqêmg>. Thirdly, since it is predictable, it could remain unwritten: <taqmg>. In fact, Pacifique makes occasional use of each of these methods of writing such sequences. This seems to be a case in which none of the variants is pernicious, and where there is no good reason to insist on a single variant. Some instances of this rule, however, seem to call for the first variant only. In a few words, the rule of contraction deletes the vowel a or o; some of these words have a q after that vowel. Thus

(45) naqanīgey 'I scoop'

has the contracted future form

(46) [nqanīkās] 'I will scoop'

and

(47) toqon'm 'I have two of (it)'

has the future form

(48) [ətqonmās] 'I'll have two'.

If we spelled these two words <nqnĪgās> and <tqnmās>, respectively, we would have no indication of what the vowel is that should be inserted. Thus, in these cases it appears advisable always to spell them with the copied vowel spelled out.

One problem can arise here: while having two or three different spellings for most such words in these cases should cause no difficulties for readers, it can pose a problem for a Micmac dictionary--the different spellings might appear in rather different places. It seems rather burdensome to list two or three different spellings (with cross-references) for each such word, so we might wish to have a standardized spelling for these types of words--either with the vowel there, or leaving it out. Since we would spell words like *nqanĪgās* and *tqonmās* with the 'copied' vowel there in any case, it seems advisable that the standardized spellings for all such words should have the 'copied' vowel there. (See appendix).

As we have discussed, spelling practice is very likely to be different depending on whether the speller is a fluent Micmac speaker or not, regardless of what system the person is using. Another example of this phenomenon is the long intervocalic consonants. For example, we find the left column (49a) as opposed to the right column (49b):

| | | | | | |
|---|---|---|---|---|---|
| (49) (a) | mijji | 'I eat' | (b) | mijipjewey | 'food' |
| | teppi | 'I'm on board' | | tepĪgey | 'I distribute' |
| | mattēm | 'I beat it' | | matn'g | 'I fight him' |
| | goqqwāl'g | 'I grab him' | | goqwey | 'what?' |
| | geggun'm | 'I have it' | | gegwāl'g | 'I slow him down' |
| | ess'm | 'I dye it' | | esamugway | 'I drink' |

The obvious way to standardize the spelling of the words in (49a) is with a double consonant, as they are spelled above. The words in (49b) will then of course be spelled with a single consonant, as above. Nevertheless, it is quite predictable that fluent writers will frequently spell the words in (49a) with only a single consonant. (For example, Pacifique usually spells these clusters with one consonant.) There are several reasons why this is predictable, but not likely to be pernicious. Just as is the case for long vs. short vowels, the long vs. short consonant distinction is phonemic, but only marginally so, in that minimal pairs are extremely difficult to find. Since the length distinction has such a low functional load, the failure to indicate long consonants is unlikely in any event to produce a different word.

Dialect differences can cause problems in orthography revision. Some evidence for differences even in Restigouche Micmac can be seen in Metallic and Delisle (1976:7), where the *j* in *Jāli* 'Charlie' is described as 'clearly voiced', as compared with Jerome's 'partially voiced' *j* in *jenu* 'giant' (Williams and Jerome: 34; this writer would describe Jerome's *j* e.g. as 'clearly unvoiced' (although unaspirated)). Similarly Metallic and Delisle (ibid.:9) describe *q* as 'always voiceless', while Jerome's *q* is 'always voiced' intervocalically. Perhaps the former is a reflection of the [?] pronunciation which some speakers give to /q/.

Dialectal variation is of several types. One type is not relevant to orthographic design. For example, in some areas the word *awti* 'road' is pronounced closer to [awkti], in some areas or styles closer to [aw?ti], and in others close to [awxti]. But all such sonorant-obstruent clusters are pronounced similarly in the respective areas, so this word should be spelled simple # <awti>, and the precise nature of the epenthetic stop is determined by the specific dialect or style in question.

Another type of variation, however, is relevant to how a word is spelled. For example, in Restigouche the word *mijji* means 'I eat'. In some areas of Nova Scotia and New Brunswick, on the other hand, the word is *mijisi*. Words such as this should be avoided in early lessons, and later introduced in the shape in which it is spoken in the particular area, perhaps with indication of the existing variants. Other semmingly similar cases, however, are more problematical. In Restigouche, 'I know it' is *gēitu*, in Nova Scotia *gejitu*. Similarly, 'they arrive' is *pēitājig* in Restigouche, but *pejitājig* elsewhere. While these appear analogous to the preceding case, the contracted form 'I will know it' is *gjitutes* in both areas. But this must be a contracted form of *gejitu-*, indicating that even in Restigouche this is the form underlying *gēitu* (which of course accounts for the voicing of the *t*). How to write this word in Restigouche is problematical. <gejitu> may be a problem for these speakers, in which case it may be necessary to treat this word as suppletive (<gjitu-> being the irregular contraction of <gēitu->) for these speakers. Williams and Jerome (1975:36) suggest <ge'itu> in such instances, and this seems to be the best solution.

In general, the Micmac situation is similar to that described by Garvin (1954:120f) for Ponapean: no dialect community is preeminent, so a compromise, preferably using the 'overall pattern' is best in questionable cases so that 'acceptance by [all] dialect communities [can] be assured.' Cf. also the discussion in Kaye (1975).

Beck (1964:156) cautions against orthographically separating the Bantu bound prefixes from the stems. This is quite analogous to such cases as Micmac *telintu* 'I sing' which has been written *teli ntu* in some systems; similarly, *-igtug* 'in' is often written as a separate word (e.g. in Williams and Jerome 1975). This needs to be checked, but it seems to me that a two-word spelling in such cases could prove confusing to readers. Words such as telātu 'I do it that way' would be especially problematical if written <tel ātu>, since there is no independent stem *ātu '?I do it'.

Micmac, like any language, has segments that occur in only a few words, such as interjections, and others which only occur in 'foreign' or borrowed words which have not yet become completely assimilated into Micmac. There is some question of how to spell such words. What most orthographies do is use the normal graphemes--perhaps in unusual combinations--to indicate the abnormal sounds (e.g. in English <tsk> or <tut> is used to spell [ʇ], a segment which appears nowhere else in the language). This seems to be quite a reasonable solution--it is not sensible to invent a new segment for such a submarginal contrast. There are similar situations in Micmac. Among the interjections is [šat] 'scat' (said to a cat)'. This seems fairly clearly to have been borrowed from French chatte, but it is used (at least in Restigouche Micmac) even by Indians who do not know French. So far as I know, the segment [š] does not occur in any other Micmac word. Probably the best way to spell this interjection is <sat>, even though this looks like it should be pronounced [sat] (there is no Micmac word [sat] for it to be confused with). In fact, this is precisely how Pacifique (1939:18) spells this word.

Another marginal segment is [h]. This segment only occurs in a few interjections ([aha] 'agreement', [ēhe] 'yes' and a few obsolete interjections) and as an occasional intervocalic alternant of /s/ in the morpheme /tes/ (e.g. /nastesin/ 'I'm caught' can be either [nastezin] or [nastehin]). In the former cases, as long as vowel sequences are not used for long vowels, we can simply spell them <aa>, <ēe>, with the understanding that between identical vowels, a phonetic [h] occurs. If vowel sequences are used for long vowels, this will cause problems, but no more so than in English. For the latter case, we can simply spell <nastesin>, and speakers will pronounce it either way (cf. *economics*, pronounced--often by the same person--both [ikənamiks] and [ekənamiks]).

The rare segment [r] occurs only in recent English borrowings (e.g. *kar* 'car', *trēyn* 'train'). This could be indicated by a rare grapheme <r> or perhaps by <l>--the [r]'s in the examples may well become [l] as the loans are assimilated--cf. *laqa!ūs* 'pitcher', *laklem* 'cream', from French *la cruche*, *la crème*, respectively. Another rare segment [ŋ] occurs, so far as I know, only in the word *mītiŋ* 'meeting', an obvious English borrowing, which has, however, been assimilated somewhat into Micmac (cf. the plural *mīgiŋ̄*). This word poses considerable problems, since [nk] sequences occur with some frequency in Micmac--thus the graphemic representation <ng> would be confusing if [k] were normally represented by <g>. If <g> is not otherwise used in Micmac spelling, however, this word could be quite conveniently spelled <miting>, and the <g> would clearly signal its 'borrowedness'. A consideration adumbrated by Francis (1970:55) is that somewhat different spelling conventions for special subsystems of the language (e.g. 'foreign' words) may be beneficial in a language. Garvin (1954:124) also feels that 'left over' letters in the alphabet of the national language can be used for foreign borrowings.

## CONCLUSIONS

While I will propose a particular orthography for Micmac, it must be concluded from my discussion above that there are many factors to be considered in developing an orthography besides the linguistic ones. In fact, as can be seen from Klima (1972) and Fidelholtz (1975), the purely linguistic considerations often themselves lead to contradictory conclusions. So even in vacuo, the 'correct' orthography in many cases is a judgement call. When the socio-cultural factors are taken into account, many further judgemental decisions become necessary. But within very broad limits indeed, any orthography acceptable to speakers can serve admirably for literacy. This doesn't mean that we should avoid making suggestions and decisions about what we feel to be better systems, but rather that we can all afford to compromise on various issues without fear that the results will be disastrous--if experience with orthographies in other languages is any guide to the probable results in the Micmac case. Certainly this is the time--before a great deal of literacy materials have been prepared in a number of competing orthographies--to make a decision on a uniform Micmac orthography. Experience has shown that a group of people who become literate in a particular orthography will strenuously resist any changes in that orthography. If a number of groups of Micmacs become literate in a variety of competing orthographies, it will then become extremely difficult, or even impossible, to standardize Micmac orthography. Now is the optimal time to standardize Micmac orthography, before such a situation arises.

It was heartening to see at the September meeting just the above-mentioned spirit of compromise among the Micmacs (and even the linguists) present. The reason was not the above one, but rather a feeling by everyone that all Micmacs should become literate in a single system, rather than having competing systems. Nevertheless, as long as the basic systems were the same, it was felt that a fair degree of regional or individual idiosyncracies could be tolerated--e.g. representing long vowels by V́ or V̀ or V̄ (or even VV); or representing [č] by <j> or <ʧ> or <tj>. The discussion in this paper should make it clear that I completely agree with this.

The orthography which all the considerations above had led me to favor is the following:

(50) | p s t j g q | m n l w (y or i) | a e i o u ' | a e i o u |

and, for occasional use in borrowed words, etc., other letters in the English alphabet, such as r, h, etc.

For nonnative learners of Micmac, simply presenting the alphabet and the phonetic values of the segments is insufficient. If (as will be true for most such persons) their native language is English, un-English combinations of segments should also be presented very early as more or less unitary combinations. For example, the Micmac combinations of obstruent consonant plus sonorant consonant are always pronounced with a syllabic sonorant consonant (in particular in word-initial position), unlike the situation in English. Therefore, such combinations as <kl>, <pl>, <tm>, etc. should be taught as specific combinations.

## APPENDIX

### ALPHABETICAL ORDER

This topic may appear almost frivolous at first glance, but in fact it is important for several reasons. For teaching purposes, it is useful to have a standardized order for presenting and practicing the Micmac alphabet. More importantly, especially for nonspeakers, it is necessary to be able to look up words in a dictionary. Considering that the Micmac orthography, whichever one is finally decided upon, will be at least somewhat similar to Pacifique's, it is evident that it will be based on the Roman alphabet. Additionally, Micmac is embedded within an Anglo-French culture, and there is a long-settled order for the Roman alphabet. Therefore, it seems reasonable to use the English (say) order, where that is relevant. The usual practice is to order letters with diacritics immediately after the corresponding undiacriticized letter. For Micmac, I would suggest a slight modification of that policy.

For letters with diacritics (e.g. <ĝ>, if that is decided on), treat them as if they had no diacritic, unless two different words would then be alphabetically identical, in which case the letter with the diacritic is order after the plain letter. The alphabetical order then should be (letters which are in dispute or only used marginally are parenthesized):

(51) a, (c), e, (ê), (g), (ĝ), i, (j), (k), l, m, n, o. (ô), p, (q), (r),
s, t, (tj), (u), (w), (y), ('), (.)

Note that all symbols which consist solely of a diacritic are ordered after all other symbols.

Another principle which I would suggest for Micmac has to do with long vowels and consonants. Since they are so marginal in Micmac, I would recommend treating double consonants as if they were single consonants--again, if two words would otherwise be identical, order the double consonant immediately after the single consonant (but before the consonant plus any diacritics)--and likewise for long vs. short vowels. The *nepāl'k* comes immediately before *nēpāl'k*, but both come before *nepilk* 'I take care of him'. Similarly, *wissey* 'nest' follows *wisawow* 'loose, diseased feces', but precedes *wīsis* 'animal'. Since *w* and *y* (if they are to be used) are almost always predictable from underlying *u* and *i* respectively, the possibility should be considered of treating <w,u> and <y,i> as being respectively indistinguishable, except where identical ordering would result, in which case *i* would precede *y* and *u* would precede *w*, respectively.

Another recommendable principle for Micmac is to disregard shwa (<'> or <ê>), again unless identical orderings would result, in which case shwa should come after all other nondiacritic segments. Of course, the reason for this, as in the earlier cases, is the predictability (or very low functional load) of the contrast. Of course, the symbol <'> which I suggest for shwa is more in keeping with this suggested principle than other, more alphabetic-looking symbols. The order for (50) above (with marginal graphemes parenthesized) is:

(52) a, e, g, (h), i, (y), j, l, m, n. o. p, q, (r), s, t, u, (w), ('), (.)

Another valuable lexicological principle is suggested by Taylor (1974:T9): 'variants [of words] should all be spelled phonetically in the dictionary.' But note the caution of Valdman (1968:322) not to confuse transcription with orthography. That is, phonologically predictable variation is beyond the province of a dictionary proper, as well as an orthography.

## NOTES

[1] This paper has benefited greatly from discussions and associations with a large number of persons. Most notable are the participants in the Conferences on Micmac Orthography held in Truro, N.S., 16-18 September 1975 and 29-30 November 1975--Mrs. Mildred Milliea, Manny Metallic, Bernie Frances, Watson Williams, Gilles Delisle, Doug Smith, and Gordon Francis. A number of other persons have helped with bibliographic sources and discussion. Most of my knowledge of Micmac is due to the tutelage of John Jerome. None of the above would, I suspect, agree with a lot of what I have said. Nevertheless, their suggestions and comments started me thinking along many of the lines developed here. I am sure that there are misinterpretations of the Micmac data and misstatements of facts, for which I apologize. The examples are intended to be illustrative and suggestive rather than definitive.

[2] In this paper, I will use brackets ([ ]) to enclose phonetic transcriptions, solidi (//) to enclose underlying forms of any degree of abstraction more abstract than the phonetic level, and angle brackets (<>) to enclose graphemic representations (i.e. spelled forms). The orthography used in this paper is the one suggested for Micmac--it will be used unless some particular point requiring some other orthographic principle is being made.

R will be used to represent the sonorant consonants in Micmac: [m, n, l, w, y]; T will represent the voiceless obstruents: [p, t, č, k, q, s], while D will represent their voiced counterparts: [b, d, ǰ, g, ɣ, z].

[3] The g̲ in this word is a rare segment in Micmac: a voiced velar continuant [ɣ].

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# ON THE TERRITORIAL DISTRIBUTION OF THE WESTERN WOODS CREE

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## I

The purpose of this paper is to assemble the data pertaining to the very late pre- and proto-historic distribution of the Western Woods Cree, and to indicate that their occupation of western Manitoba, Saskatchewan and possibly Alberta belongs to the pre-contact period. The period in question is that of the middle seventeenth to middle eighteenth centuries, as European exploration advanced to the west. It is prompted by continuing reference by ethnologists to a westward movement of Cree in the late seventeenth century and later from the region roughly bounded by Hudson Bay, Lake Superior and Lake Winnipeg. This movement is usually attributed to the consequences of the fur trade: the introduction of the gun that gave military superiority, the depletion of beaver and other fur bearing animals, and more recently to a recognition of the extent to which the big game population had been reduced (Bishop 1970; 1974:183-227; Ray 1974:117-24). There is ample historical documentation for the movement of the Swampy Cree to the southwest under these conditions (Mandelbaum 1940), but they cannot alone be responsible for the population of all western regions within such a remarkably brief period. It is suggested that it was the term Cree (and its variants) that spread west and was applied to Cree groups known by other names, or unknown until the relatively late exploration of the Canadian Northwest, although it is not denied that boundaries fluctuated with temporary technological superiority, or conversely, as epidemic disease caused territorial contractions.[1]

The Western Woods Cree exist in three, and possibly four, major divisions: the Maskegon or Swampy Cree from the Hudson Bay Lowlands through the upper Lake Winnipeg region and a westward extension to the vicinity of Cumberland House in modern Saskatchewan; the Rocky Cree on the lower Churchill River drainage and west of the Nelson River, in the northern parts of Manitoba and Saskatchewan; the "Athapuscow" or Cree near Lake Athabasca; and the Strongwoods, Thickwoods, or Bois Fort Cree in northern Alberta. It is primarily with the latter three groups that I shall be concerned. In general, the territory they have occupied and exploited in historical times is that of the full boreal forest, with some use of the forest transitional zones on the northeastern and southwestern edges. The North Saskatchewan River, following Mandelbaum (1940:165), may be taken as the zone of transition from Woods to Plains Cree, and Ray (1974) has recently shown how occupation of the transitional aspen forest provided for seasonal use of the resources of both the plains and full boreal forest. While this paper is concerned solely with the historical question of Cree distribution, it is basic to an understanding of a fundamental adaptation, including kinship, marriage and local or band groupings, to an environment.

Anthropological study of the groups in question has been almost totally neglected, with the exception of the Swampy Cree near Hudson and James Bays. This writer worked with the Rocky Cree around Reindeer Lake (Smith 1968, 1975a), but the Strongwoods Cree are known only from Mandelbaum's references, as they were marginal to the Plains Cree that he studied, and the "Athapuscow" Cree are known only from brief historical references. A dialect survey (cited in Wolfart 1973:7-10) provides important but limited data, and there is some archaeological evidence available for part of the area. In addition, there are a number of early historical works of considerable value.

Mooney and Thomas (1907:10, I:359-60) wrote, based on the earlier literature, that the Cree's "former habitat was in Manitoba and Saskatchewan, between the Red and Saskatchewan rivers. They ranged northeastward down Nelson River, to the vicinity of Hudson Bay, and northwestward almost to Athabaska lake." After the alliance with the Assiniboin, they attacked and drove southwestward the Siksika, or Blackfeet, and other tribes. As a statemen by two outstanding scholars, published in the authoritative Handbook, it held great weight. In various forms this statement of early historic westward conquest has been restated or accepted by others, particularly Curtis (1928, XVIII:58), Jenness (1932:284) and Kroeber (1939:83) in syntheses that have equally been standard reference works. Subsequently, this view has been expressed by recent scholars, including (randomly) Ewers (Denig 1961:101, fn.5), Hlady (1964:24-31; 1970:96), Canada (1967:1), Fischer (1973:134), Helm and Leacock (1973:343-45) and Ray (1974:3-26). The success of the Cree in moving westward is attributed to earlier possession of firearms that gave them an enormous advantage.

The problem of Cree pre- and proto-historic distribution is related to that of the Chipewyan immediately to the north, which has been considered by Gillespie (1975) and Smith (1975b). Gillespie has established that the Chipewyan occupation of the full boreal forest south of Lake Athabasca to the Churchill River was a late eighteenth and early nineteenth century movement. Gillespie argues that the thesis of Cree historic expansion stems from a misinterpretation of a passage in Alexander Mackenzie's (1927: 81-2) account of his explorations of 1789, in which he wrote:

> ...the Knisteneaux [Cree], when they first came to this country, and either destroyed or drove back the natives, whom they held in contempt, on many accounts, but particularly for their ignorance in hunting the beaver.... [the Portage de Traite] was, at that time, the utmost of their conquest of warfaring progress west, and is latitude 55.25 North, and longitude 103 3/4 West.

Petitot (1885:47), in another early and widely disseminated paper, added to the misunderstanding. He gave western Cree distribution in the French period as including Isle à la Crosse and Cold Lake, the latter on the Saskatchewan-Alberta border. He placed the Chipewyan to the west, along the Peace River, asserting that it was not until later that they moved to the lands they occupied in the nineteenth century.

Gillespie then cites another passage from Mackenzie (pp. 86-7) that describes the Cree as the traditional occupants of the Churchill River drainage system:

> Who the original people were that were driven from it, when conquered by the Knisteneaux [Cree], is not now known, as not a single vestige remains of them. The latter, and the Chipewyans, are the only people that have been known here, and it is evident that the last-mentioned consider themselves as strangers, and seldom remain longer than three or four years, without visiting their relations and friends in the barren ground, which they term their native country.

Gillespie then turned her attention to the historic movement of the Chipewyan southward into the full boreal forest, and their changing relations to the Cree.

## II

An examination of the archaeological, linguistic, and historical literature indicates that the Cree occupied before contact the lands in general that they occupied during the full historic period.

The archaeological record is limited, but valuable. Wright (1965) surveyed and excavated sites at Southern Indian Lake, Manitoba, which is not and historically was occupied by Cree. There he found Selkirk pottery, typically associated with Cree, dating from the tenth century A.D. to the onset of historical times. Downes' (1948) survey of Reindeer Lake in northern Manitoba and Saskatchewan reveals the same type of pottery, which is also found at Lac la Ronge, and as far west as Isle à la Crosse and Cold Lake and north to Black Lake (J.V. Wright 1975:139, 142; personal communication). Other Archaeological investigations in northern Manitoba (Hlady 1970;

Nash 1970) further indicate Cree occupation of the Churchill drainage and perhaps somewhat to the north. With the exception of Wright's (1975) report on Lake Athabasca area, nothing has been published on the archaeology of northern Alberta, although research is currently underway or planned.

Linguistic data are also instructive, Cree dialect distribution, based on earlier sources and the Summer Institute of Linguistics, are summarized by Wolfart (1973:7-10 and maps 1-3). The dialect distinctions are based on the reflect of Proto-Algonkian */θ/. For the groups of concern here, the distribution follows: The Maskegon or Swampy Cree are characterized by the /n/ dialect, from Hudson Bay, south of the Nelson River drainage, to Cumberland House on the Saskatchewan River. The Rocky Cree are identified by the /ð/ dialect, north of the Nelson River, along the Churchill River drainage in northern Manitoba and Saskatchewan to Lake Athabasca at Fort Chipewyan (Smith 1975a; Wolfart 1973:9). The Strongwoods or Bois Fort Cree, identified by Mandelbaum as north of the North Saskatchewan River in Alberta, have the /y/ dialect, which is also the dialect of the Plains Cree. On Wolfart's (1973:9) dialect map, an anamolous /r/ dialect is shown in northwestern Saskatchewan; it appears otherwise only south of James Bay. I should strongly note that the identification of dialect group with self identified groups - Maskegon, Rocky and Strongwoods Cree - is far from certain. It is true for the Maskegon - Rocky Cree boundary in northern Manitoba (Smith 1975a), but there is no anthropological data from the west to either confirm or deny this suggestion.

Some degree of time depth is at least suggested by the existence of five dialect areas west of Hudson Bay. At least two, /ð/ and /n/, are noted in sources from the eighteenth century in the area around Hudson Bay, although not until late in that century was there major European exploration and settlement in the interior west of the Bay. There are apparently no linguistic studies to indicate the possible time depth involved, nor indications of mutual comprehension of dialect speakers. The dialect distribution suggests some time depth, but in itself is not conclusive. Considered in conjunction with the archaeological and historical data, it is however, plausible.

Although marginal to the focus of this paper, the predominance of the /y/ dialect among the Plains Cree suggests a significant Strongwoods Cree element in the composition of the former group; Mandelbaum (1940:169-187) has documented the south and westward migration of the Maskegon, but there is no published explanation for the apparent predominance of /y/ rather than /n/.

The historical literature of the period of exploration is important, although variable in scope and detail, and as the time of exploration shifted from the seventeenth century in the east to the latter eighteenth century in the west.

The earliest references to the Cree occur in the *Jesuit Relations* (J.R.) of the seventeenth century, under the name *Kilistinons*, and subsequently in dialectal variations (*Kinistinon*, *Kiristinon*) probably reflecting the dialect differences of Ojibwa-Algonkin-Ottawa informants. These terms, to which *Knisteneaux*, *Christinaux* and others should be added, referred to specific groups.[2] In 1640 the *Kilistinons* were said to "dwell on the rivers of the north sea where Nipissings go to trade with them" (JR 18:229). The *Relation* of 1656-58 (JR 44:249) note four nations[3] of Kilistinons, three of which the editor, R.G. Thwaites, located west of James Bay:

> Those of the first [nation] are called the Alimibegouek Kilistinons [Lake Nipigon Cree]; of the second the Kilistinons of Attaouabouscatouek Bay [Cree residing west of James Bay]; of the third, the Kilistonons of the Nipisiriniens, because the Nipisiriniens [Nipissings] discovered their country whither they resort to trade or barter goods [Cree between Lake Nipigon and Moose River].

In addition to the Kilistinons, the Monsoni (Moose) were initially reported south of James Bay, (JR 57:203 ), and the /r/ dialect now there may represent their descendants, although La Potherie (Tyrell 1931:262-63) uses the term Monsoni subsequently for people further to the west of James Bay. The people to the west of James Bay and near Hudson Bay were referred to by the French at York Fort, or Fort Bourbon as it was known during the French occupation of 1694-1714, as the peoples of the Savannahs (La Potherie, in Tyrell 1931:263; Jérémie 1926:passim). The term Maskegonhirinis or Maskegonehirinis (La Potherie, in Tyrell 1931:265), related to Maskegon, was also used for the Cree near York Fort. On the south, Father Allouez (JR 51:57) reported that part of Cree subsistence was based on gathering "wild oats" (i.e., wild rice), implying a rather southerly distribution at least in summer. In the 1660's, Perrot (Blair 1911, I:107-8) reported that the Cree often frequented the region around the shores of Lake Superior, and Radisson (1961:passim) encountered Cree on the upper Mississippi River.[4] Later French exploration in the west, e.g., that of LaVerendrye (Burpee 1927), did not extend sufficiently north to be of interest here.

While the French of the seventeenth and early eighteenth century identified nations or bands by specific terms, the English at the forts on Hudson Bay during the early period generally tended to refer to the Cree simply as Southern or Uplands Indians, in contrast to the Northern Indians, who were the Chipewyan (Knight 1932:passim). Later, some more specific terms were used, but on the whole the English tendency was to "lump".

Kelsey (1929), in his explorations of 1690-91, used another common term "Home Guard" (i.e., Cree attached to the HBC forts), but added "Nayhathaways", subsequently used, in variant spellings, for the Cree of northwestern Manitoba and northern Saskatchewan. Burpee (1907:307) argued that Kelsey was on the

Churchill drainage. Kelsey (1929:8, 18) makes it clear that the "Nayhathaways" were a socially distinct group, in contrast to the Southern Indians trading into York Fort.

To the HBC officials at Fort Churchill in the early eighteenth century, the Cree living on the lower Churchill River were known as the "Mishenepe" Indians, after Mishenepe (or Missinipi), or "big water", their name for the river and for Southern Indian Lake. Unfortunately, aside from Kelsey's inland exploration, there was a long hiatus before the later penetration of the western interior. Hendry's journey of 1754-55 (Burpee 1907) from York Fort to the Blackfeet apparently took him too far south - or at least he did not refer specifically to the Cree, other than those who accompanied him. In 1772-73 (Burpee 1908), Matthew Cocking encountered some Cree, accompanied by Assiniboin, somewhere along the North Saskatchewan travelling east, but provided too few pertinent observations.

In the narrative of his great journeys of 1769 to 1772, Samuel Hearne (1911:146, 161, 223) differentiated between the "Athapuscow", "Neheaway", and "Southern" Indians. The "Athapuscow" were Cree living approximately around and to the north of Lake Athabasca, perhaps as far as the south shore of Great Slave Lake, with whom Hearne's Chipewyan guide, the great leader Motonabbee, had established peace in the 1750's (Hearne 1911:331-33).[5] The "Neheaway's" (perhaps /y/ dialect) location was not specificied but presumably to the south of the specifically designated "Athapuscow Tribe". The "Athapuscow" of Hearne are the "Araubaska" of Pond (Innis 1930) and the /r/ dialect of Lacombe (1874). "Southern" Indians referred to Cree speakers in general, but was also used somewhat more specifically (in this region) for those trading into York Fort.

The "Athapuscow" require additional comment concerning identification and location. In Cree, the term refers to a river mouth characterized by reeds or grass, and as the designation for a group first occurs in the Hudson's Bay Archives in 1755. Then Ferdinand Jacobs, chief at Fort Churchill, wrote to James Isham, Governor at York Fort, that "many of y$^{e}$ Athapuscow Indians not coming down to Trade" (HBC B 42/b/16).[6] B.C. Gillespie (personal communication) finds no references to them in the York Fort archives, and they seem to be accorded separate recognition at Churchill only after the HBC became concerned with the competition of the French, and, after 1763, with the Montreal traders. Hearne (1911:331), in his discussion of Matonabbee clearly identified the "Athapuscow Tribe" as Cree, when he wrote that Governor Jacobs at Churchill had sent Matonabbee "as an Ambassador and Mediator between the Northern Indians [Chipewyan] and the Athapuscow Tribe", who had always been at war with one another. Later, (pp. 331-3) the Athapuscow are identified as Cree, as the former term and "Southern Indian" are used interchangeably. (Other references that identify Athapuscow as Southern Indians, in contrast to Northern, are in Hearne, pp. 53, 146, 161, 200, 223, 263, 266, 267.) The Athapuscow ceased to visit Churchill after Peter Pond and his associates settled in the Athabasca territory in 1778 (Glover 1958:72, fn.), and numerous trading posts were established.

Given that the Athapuscow tribe was one of the major Cree divisions, their precise location is yet to be established. There is no question they were a long distance inland, for they are not reported at York (and thus not on the Saskatchewan-Nelson drainage system), and only between 1755 and 1778 at Churchill, thus, one infers, somewhere with access to the Churchill River drainage. Hearne (1911:261, 255) described the Anthapuscow Country as well wooded, with moose and beaver abundant and woods bison present; he noted that moose were never found in Northern Indian territory. Matonabbee at the time they were on the east arm of Great Slave Lake, which Hearne thought was Lake Athabasca, wanted to go southwest in hopes of meeting some Athapuscow Indians (Hearne 1911:262-63). The Athapuscow were then at war with the Dogribs (pp. 263, 265). Somewhat later, in 1789, Mackenzie (1927:137) wrote that the Slave River derived its name from those Indians who had been driven from it by "Knisteneaux", and that the Knisteneaux had recently driven Slave Indians from islands in Great Slave Lake (p. 152). Peter Pond, who had wintered south of Lake Athabaska in 1778, placed the "Araubaska" Indians south of the lake, but the Chipewyan far to the northeast, in his map of 1785. Arrowsmith's map of 1819, based on David Thompson's of 1795, shows the "Athapescow" to the south of the lake, with "Chepawayan" immediately south of Great Slave Lake; immediately south of the "Athapescow" were other "Chepewayan" and "Nahathaway". The Cree and Chipewyan distributions of the late eighteenth century reflect the consequences of peaceful relations, the after-effects of the smallpox epidemics, and the penetration of the Chipewyan into the full boreal forest (Gillespie 1975, esp. Figs. 2, 4).

Archaeologically, the distribution of Cree north of Lake Athabasca is not substantiated. Wright's (1975) exploration of the Lake Athabasca region indicates only Chipewyan sites to the north of the lake, and no material of Cree association was found. One interpretation of the contrast between the archaeological and historical records is that, in this instance, Cree expansion north of Lake Athabasca was in the proto-historic period, i.e., immediately prior to direct European contact in the region in question. One must also emphasize that boundaries were vague, with "disputed territory" as a no-man's land separating hostile groups. In a long term perspective, boundaries doubtless varied as exploitative ranges changed in response to environmental change.

By the latter quarter of the eighteenth century Chipewyan were beginning to move south into the Cree lands of the full boreal forest between Lake Athabasca and the Churchill after the HBC had established peace between the two hitherto warring groups (Gillespie 1975), and the "Montreal Pedlars", later to be amalgamated as the North West Company, began to enter the western forest. Among the earliest of these to leave a record was Peter Pond (Innis 1930), whose explorations and trade extended north to Lake Athabasca and west to Portage La Loche and Isle à la Crosse in far northwestern Saskatchewan, and Elk River in Alberta. At Isle à la Crosse Pond "saw a vast concourse of the Knisteneaux and Chepawyan tribes" (Innis 1930:81, quoting Alexander Mackenzie), and Alexander Henry the Elder (Ibid:82) noted that "the natives inhabiting these parts, seems separated, and unconnected with the Southern Indians, their languages different but little from the kristinay...." Pond (pp. 119-20) did note, however, that "the peoples of the west [Beaver?] had been forced back a great distance during the past 40 years."

David Thompson (1916), explored much of the western interior in the 1790s, and on the basis of six years in the area of the Cree on behalf of the Hudson's Bay and North West Companies made a number of pertinent observations. The Cree of the western boreal forest were the Nahathaway, their name for themselves. Their territory extended south of 56° North latitude, south to the Saskatchewan River, and west to the Rocky Mountains. The dialect is "th" [/ð/], but changes to /y/ in the south, where it "becomes almost a different language" (p. 79). Speaking specifically of the Reed Lake area, north of the Saskatchewan River, but emphasizing that his comments applied "everywhere", he wrote (p. 108) that "The Natives are Nahathaway Indians, whose fathers from time beyond any tradition, have hunted these lands...."

Thompson further alludes to terminology. Nahathaway is their native name (pp. 78-9), but he parenthetically notes (p. 79):

> These people by the French Canadians, who are all without the least education, in their jargon call them "Krees" a name which none of the Indians can pronounce; this name appears to be taken from "Keethisteno" so called by one of their tribes and which the french pronounce "Kristeno", and by contraction Krees (R, rough, cannot be pronounced by any Native) these tribes are separated into many tribes or families, under different names, but all speaking dialects of the same language, which extends over this stony region [i.e., Pre-Cambrian Shield], and along the Atlantic coast southward to the Delaware River in the United States, (the language of the Delaware Indians being a dialect of the parent Nahathaway) and by the Saskatchewan River westward, to the Rocky Mountains.

J.B. Tyrell, Thompson's editor, observes in a footnote (p. 79) that Nahathaway is a term used for themselves by the Cree who live around Isle à la Crosse and the upper waters of the Churchill River, i.e., more or less what Rossignol (1939) and I (Smith 1975a) have termed Rocky Cree.[7] Still no comment is made to referential terms or dialect of northern and western Alberta.

Perhaps the greatest significance of Thompson's observations is that, first, he notes the distribution of the Western Woods Cree as far as the Rockies, secondly, he provides evidence that they have "always" been in this area (even if boundaries have expanded or contracted at various times), and, finally, that the once specific name Cree was being indiscriminately applied to groups hitherto known by other names.

Mackenzie (1927) was apparently the first Englishman to extend the term Knisteneaux to all those we now know as Cree, making no distinctions within the category. He described the extent of their territory, in the west, about 1790 as follows (pp. 97-8):

> It [the boundary] then proceeds till it strikes the middle part of the river Winnipic, following that water through

> the lake Winnipic, to the discharge of the Saskatchiwine into it; from thence it accompanies the latter to Fort George, when the line, striking by the head of the Beaver river to the Elk River,[8] runs along its banks to its discharge in the Lake of the Hills [Lake Athabasca]; from which it may be carried back east, to the Isle à La Crosse, and so on to Churchill by the Missinipi.

Alexander Henry the Younger (Coues 1897, II:510) noted Cree as far west as the Peace River, the name of which derived from a peace made there between the Cree and Beaver Indians (Mackenzie 1927:240). According to Mackenzie:

> When this country was formerly invaded by the Knisteneaux, they found the Beaver Indians inhabiting the land about Portage la Loche; and the adjoining tribe were those whom they called Slaves. They drove both these tribes before them; when the latter proceeded down the river from the Lake of the Hills, in consequence of which that part of it obtained the name of the Slave River. The former proceeded up the river; and when the Knisteneaux made peace with them, this place was settled to be the boundary.

Thus, at the onset of the recorded European history of the Western Woods Cree, they extended as far west as the foothills of the Rocky Mountains. The question remains, however, as to the length of occupation of the area. On this point, Mackenzie is ambiguous, for he refers both generally to a westward involvement of the Knisteneaux and to specific expansion against neighboring groups. Writing generally of Indians in the west, he noted (p. 77 "...and, as for the Knisteneaux, there is no question of their having been, and continuing to be, invaders of this country, from the eastward".

This, and other similar remarks, may be taken either to refer to recent movements, or those of some vague or distant past. His comments about the establishment of peace between the Cree and Beaver, coupled with Pond's remark that the people of the west had been forced back within the past forty years (i.e., roughly between 1730 and the 1770s) indicate that on the western front expansion was in the very late "prehistoric" period. Thompson's statement, however, indicates that northern Manitoba and Saskatchewan were historically Cree.

The Cree of northern Alberta (and perhaps of northwestern Saskatchewan) are sometimes referred to as the Strongwoods, Thickwoods, or Bois Fort Indians (Curtis 1928, XVIII:55-87), but early references to that name are very rare and the reference unclear. These are the people identified on Pond's 1785 map (Gillespie 1975, Fig. 1) as the "Beaver River Indians". Hind (1968, II:149, 159), citing the 1856 report of the Select Committee on the Hudson's Bay Company, reported that there were 35,000 "Thickwoods Indians on the east side of the Rocky Mountains", a figure that apparently includes all Western Woods Cree and perhaps the Plains Cree. He also noted (p. 152) that "The Strongwoods Cree about Edmonton Mr. Roward [an HBC trader] estimated at 400 tents, at 10 per tent, = 4,000". Hayden (1853) did not use any of these terms, but refers to 200 lodges of Cree "scattered along Lac de L'Isle Croix", and Wissler (1936:3-18) used this figure in estimating his "Thickwoods Cree" population. Evidently Wissler had no information available on northern Alberta.

On the north, Cree territory contracted (Gillespie 1975). At the onset of recorded history, they occupied the Churchill drainage as far north as Reindeer and Black Lakes, or about 58°N. latitude, and as far as Lake Athabasca and perhaps to the north. After the HBC established peace between the Cree and Chipewyan, the latter began to move south to exploit the greater fur resources of the full boreal forest. Their expansion was facilitated by the disastrous effects of the great smallpox epidemic of 1781, after which the surviving Cree appear to have concentrated nearer the Churchill River. Cree movement to the north in the nineteenth and twentieth centuries has been a return to ancestral lands (Smith 1975a).

## III

The limited archaeological data suggest that late prehistoric Cree occupation extended to western Saskatchewan, and future surveys or excavations may establish the boundary there or in Alberta. Cree dialect distribution is in accord with this interpretation of the archaeological data. The historical evidence is equally consistent: as European exploration proceeded westward, Cree were invariably the inhabitants as far west as the Rocky Mountains. Only in the far west, i.e., Alberta, and north of Lake Athabasca are there major questions about the boundary. There, the testimony of Pond is that expansion to the Peace River was recent, but the area of Cree conquest indeterminate (Fig. 1, 2).

Within the boreal forest west of Hudson Bay, there were a number of socially identified Cree groups. These included the Maskegon (/n/ dialect) south and generally east of the Nelson River drainage; the Rocky Cree (/ð/ dialect) of the Churchill River drainage, probably to be identified with the Nehathaway" of the latter eighteenth century explorers; the "Athapuscow Tribe" (/r/ dialect) in northwestern Saskatchewan, probably around Lake Athabaska; and the Strongwoods Cree (/y/ dialect) of northern Alberta. Those of the far west may have initially traded through Cree intermediaries, and hence may remain historically poorly known even after the HBC archives have been examined. Of these, reasonably adequate early ethnographic data are available only for the Hudson Bay Lowlands.

Within the boreal forest, probably flexible boundaries of the various Cree groups expanded or contracted in response to population growth, reaction to epidemic disease, the dynamics of the forest ecology, new perceptions and patterns of exploitation with the advent of European technology, and other factors. To explain Western Woods Cree distribution by a war of conquest that extended their western boundary some 500 miles between the advent of the Europeans in the late seventeenth century and the middle of the eighteenth century, a Blitzkrieg, does not seem credible. Cree began to obtain muskets from the French and English perhaps by 1670, but probably in relatively small numbers, and one may question the number traded to the western periphery. The muzzle-loading, single shot musket certainly gave a psychological advantage to the Cree over their enemies without fire arms, but the early trade guns were of inferior quality and the lock mechanism (especially the hammer) were prone to break in the severe cold of Subarctic winter. Some territorial conquest did occur on the border with the Beaver and Slave on the west, South onto the Plains, and temporarily north of Lake Athabasca, but warfare in the Subarctic was not, in general, for conquest (Slobodin 1960; Gillespie 1975).

The ultimate determination of pre-contract Cree boundaries and time depth requires archaeological research in this much neglected area; happily, that is now beginning.

## NOTES

[1] I am indebted to Drs. C.A. Bishop, G.M. Day, and J.V. Wright for their helpful criticism of the initial draft of this paper. The interpretation presented here is, of course, my own.

[2] For the tribal synonomy, see Mooney and Thomas (1907, I:361-2).

[3] The term nation was, and is still, used to refer to a group characterized by a sense of self identity deriving from common language, culture, and history. It does not, in its original usage, imply a political entity.

[4] The southern distribution of the Cree is a matter of current controversy. C.A. Bishop and M.E. Smith (1975) have recently presented evidence for the attribution of the Blackduck focus to Siouan speakers, while other scholars (e.g., Dawson 1974, Wright 1965, 1972) argue for Northern Algonkian affinities.

[5] The persistence of an /r/ dialect near Lake Athabasca suggests that the "Athapuscow" Cree may still survive as a socially recognized ethnic entity.

[6] Published by permission of the Hudson's Bay Company.

[7] Specific Cree groups, as well as those of other Northern Algonkians and Athapaskans, were commonly referred to by their neighbors by some characteristic of the territory they occupied. Thus the Swampy Cree were those of the lands where muskeg were abundant, the Rocky Cree were those of the Pre-Cambrian Shield with its striking rock formations, and the Strongwoods Cree apparently of the "Mixedwood" zone of the boreal forest (Rowe 1972:36).

[8] Mackenzie may have meant the modern Wapiti River; the modern Elk River is too far south to be anything but Plains Cree. Fort George was a North West Company post in the 1790s and early 1800s, located near present Fort Vermillion.

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# ALGONQUIAN LANGUAGES IN INDIAN EDUCATION

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Until recently, formal education for Indian Children in North America has been almost entirely conducted by whites on white educational models and has been assimilationist in intent. In Canada until the middle of this century, the government department responsible for Indians contracted out the job of educating Indians to religious groups and mostly left them alone to deal with the situation as they saw fit. In the U.S., despite the Meriam Report of 1928 (United States, 1928), the pattern was pretty much the same. In the 1950's and 1960's in Canada, although the aim of cultural assimilation remained, the government policy on Indian education changed to stress school integration with the majority society rather than isolation as before. However, it continued to be true of education for Indians that (1) white educators applied their model of education, virtually unadulterated, in Indian schools, (2) Indian people had no effective voice in decisions in Indian education and (3) it was rare for appeals to be made from the educational establishment to the academic community for research that might lead to alterations in the education of Indians that would make it more suitable to Indians' particular needs. In fact, the agreements for the integration of Indian children into the provincial schools signed since the 1950's have pointedly stated that the education provided for Indian children was to be in no way different from that for other children.

During the 1960's a number of things happened to change the face of education for minority groups in general and for Indians in particular. In the U.S. there was the War on Poverty and the provision of funds for education programs through the Elementary and Secondary Education Act (United States, 1965) and the Johnson-O'Malley Act (United States, 1968). In Canada there was the Report of the Royal Commission on Bilingualism and Biculturalism (Canada, 1970), the Hawthorn Report called A Survey of the Contemporary Indians of Canada (Canada, 1967), Jean Chretien's statement of federal policy on Indian affairs (Canada, 1969), and the National Indian Brotherhood's reply to it in Indian Control of Indian Education (National Indian Brotherhood, 1972). These and other movements and events have been instrumental in bringing about significant changes in Indian education. One is that native groups are demanding and to some extent getting recognition for the facts, first that their needs in education are different from those of the majority culture and secondly that they should have some control over the education of their children. Another change is that education in North America is no longer the immutable monolith it once was. Educators have come to realize to some extent that one model for education is not necessarily applicable in all cases. I am referring here particularly to the acceptance of various forms of bilingual education all across the continent. Funding has been provided and regulations have been adjusted to permit alterations in the basic structure of education for Indians and other groups. These alterations have been in the areas of recognition of community languages and cultural practices and in community participation in school activities and decision making. One other change is that Indians and educators are looking to the academic community more than they did before for information and advice on the changes to be made. Also, academics are becoming more involved in contemporary Indian events and in education. I am not suggesting here that Indians, academics and educators are virtually beating paths to each other's doors. I do feel, however, that the interests and resources of all three groups have been drawn closer together. The point is that Indian education is now in a position to be other than assimilationist and to take forms other than the standard white model--the challenge is in determining what these forms should be and how best they can be implemented.

The aspect of change in Indian education that I will focus on here is the introduction of Algonquian languages into schools for Algonquian children. Today many schools recognize the first or ancestral languages of the children in some way or another. Plans are being made for more Indian schools to do this and for the expansion of present programs. The form these programs take vary as widely as the communities they occur in. In this paper, I will look at generalizations that can be made about all of these programs and attempt to raise questions relevant to the concerns of native people, educators and the academic community. It is hoped that these questions will provide some useful focal points for those working on individual Algonquian school language programs and for those involved in research or policy making in Indian education.

The two main generalizations I would like to propose involve objectives for Algonquian school language programs. I suggest that one or both of these objectives could be considered to be implicit in every program. The first is that the program is aimed at improving the Indian child's overall school achievement in the usual North American sense of the expression. The other is that the program is an attempt at changing the language habits of the child. In order to discuss these two objectives I will sometimes consider separately the programs on the one hand for children who come to school speaking only or predominantly an Algonquian language, and on the other hand for children who speak only or predominantly an official language.

Looking at the first objective, one can ask whether it is valid to introduce an Algonquian language into the school to improve Indian children's school achievement. There are no apparent reasons why not _if_ the community wants improved school achievement for their children and _if_ the community is willing to permit the use of their native or ancestral language in a school context.

These two conditions are often taken for granted, but these questions must be asked at the outset of the planning of any Indian language program with this objective. If the community doesn't want improved school achievement for the children, then perhaps alternative forms of education, which have aims other than primarily academic success, ought to be considered. This particular issue is beyond the scope of this discussion but it must be kept open nonetheless. There have been cases in which many people in a community have not been in favour of using the Indian language in the school, but have been won over to the idea after a year or two of successful running of the program. It still remains a dangerous and unethical step, however, to attempt to use the language against the community's wishes.

The next major question is, if an Algonquian language is introduced into a school to improve the children's school achievement, will it work. I am assuming here that school achievement involves fluency in the official language as well as mastery of the work in the various subject areas. Here we will have to look at the two groups, the mainly Algonquian speaking and the mainly official language speaking, separately. For the mainly Algonquian-speaking children there are many ways in which their native language can be introduced into the school. Present programs vary widely in the amount of the language used and the contexts it is set in. Some permit a teacher aide to use the children's first language to make the children feel more at home in the classroom but not to actually teach lessons. Other schools use or propose to use the first language as the medium of instruction for several years of school and literacy is first introduced in the Indian language. The children's first language can be used to create a more relaxed atmosphere in the classroom, to aid in the introduction of the official language, to teach or reinforce the teaching of regular school subjects, or as a subject itself. There are many possible formats for introducing the language into the school, particularly if bilingual or bicultural maintenance is also an objective. The selection of a format for any particular school depends on a great many factors, for example the availability of appropriate staff and curriculum materials, the type of school, legal restrictions on the use of certain languages as medium of instruction and so on.

There have been no formal, published evaluations, as far as I know, of any of the programs using Algonquian languages to improve children's school achievement. Studies done on similar programs for speakers of other languages show mixed results. For example, research comparing the Rough Rock Community School with a government run school indicated that the children at Rough Rock were not doing as well academically (United States, 1969). At Rough Rock, the children, who are almost all monolingual in Navaho when they come to school, are taught Navaho related subjects in Navaho. They are encouraged to use Navaho at any time in school when they feel that their English is not adequate to express what they want. The comparison school did not give the Navaho language the same roles in the classroom that Rough Rock did. According to a study by Modiano (Modiano, 1966, 1973), Indian children in Mexico learned to read Spanish better than their comparison group if they were first taught to read in their native language. There are literally hundreds of other studies with elements that could be compared with those of some of the Algonquian language programs. Within the time constraints of this paper, it is impossible to detail the individual Algonquian language programs and make suitable comparisons. It is perhaps enough here to say that there is no reason to be pessimistic about the possibility that some Algonquian language programs could improve Algonquian-speaking children's school achievement. On the other hand, there are many factors such as community attitudes to the school and the language, availability of suitable staff and materials, and the design of the program itself that could compromise this aim.

The language programs for children of Algonquian background who have lost or are quickly losing their ancestral language are considerably different. They are generally based on the hypothesis that the introduction of the ancestral language into the school will improve the child's self image as an Indian and that such an improvement will be reflected in his success in his regular school work. Some schools teach only a few words or expressions. Others have launched into ambitious Algonquian as a second language program. On anecdotal evidence it appears to me that these types of programs often meet with considerable success in the immediate objective of improving the child's self image. But the effect this has on his performance in his regular school work is an open question. Also, this works only under certain conditions. One might assume that the quality of the program and the amount of new linguistic material that the children learn would be crucial. It is my impression that this is not the case. It is the fact that the language is recognized in the school that seems to be important. However, if the program is offered in a mixed school to both Indian and non-Indian children, and if the non-Indian children are better school achievers in general, it is possible that the non-Indian children will do better than the Indian children in the language program. The effects on the Indian children's self image are then likely to be negative rather than positive. This would imply that the development and implementation of a good Algonquian as a second language program might be an ambitious end in itself, but its success in improving the child's school achievement or even self image is far from guaranteed. The interesting question of the role of bilingualism in biculturalism is obviously relevant here. If there is any suggestion that an Algonquian language program might have negative effects on the children's self concept, then perhaps a cultural program with little or no language included might be just as effective and safer in the long run. For further exploration of the possibilities in this direction, a more thorough discussion is available in Miller, 1972.

The second objective for Algonquian language programs to be considered here is that the program could attempt to change the child's language habits. By this I mean that the school would attempt to make him bilingual if he were monolingual in the official language, or to make him literate in an Algonquian language, or to maintain his use of an Algonquian language in a case where he was in danger of losing it, or to expand the scope of his use of the language in areas he might not have without the school program. Is this a valid objective? Certainly this type of objective is highly approved of in schools for majority culture children. The crucial factor in the Indian school situation would probably be the extent to which the school is seen by the community to be an alien intrusion. It is easy to imagine a community that would feel that such an objective on the part of the school was highly presumptuous. On the other hand, the fact that there are many Algonquian language programs today is witness in large part to the fact that many communities have asked for such programs with that kind of objective in view.

Why ask the school to take on this task? There are several possible answers suitable to different circumstances. One is that, with a background of centuries of experience with white education, many Indian people have come to believe that school is the place where certain things are learned, incredible as this may seem. Literacy is definitely one of these. Another answer is that the school provides the facilities for the community to launch a movement it is interested in. A pessimistic view is that Indian parents would like to see these changes take place in their children's language use but are unwilling, unable or afraid to take on the responsibility. In some cases it seems that the community sees such a program as an entry point into the school for themselves. It could also be seen as an attempt to fight fire with fire if the school is considered to be the major factor in the shift towards the official language.

Is an Algonquian language program likely to succeed if its objective is to change the child's language habits? Olga Kuplowsky, in her review of the literature on bilingual education, distilled the criteria for success in language learning to need, desire and practice (Kuplowsky, 1971). If we assume that the school program could provide adequate practice, will the child feel that there is a need for him to learn, or will there be any incentive for him to learn--even a negative incentive? This depends greatly on the language use patterns of the community and the status of the language in the estimation of the community. An Algonquian literacy program in a mainly Algonquian language speaking community is probably the most likely to succeed, particularly if there is a tradition of literacy in the community. On the other hand, an Algonquian as a second language program aimed at producing complete bilinguals in a virtually monolingual official language speaking community is much more of a risk. This points to the necessity of setting realistic goals with regards to the amount and type of the language to be learned.

Establishing realistic goals involves determining the community's view on the major purpose of the language for the child. If the purpose is to give him a feeling of his identity as an Indian, then, as was noted before, perhaps a few significant words or expressions are sufficient. If communication with older people in the community is important, then language learning might be restricted to a certain domain of the language or might emphasize passive understanding more than speaking ability. The same might be true if participation in religious or cultural events was important. If continuity of tradition is required, then the program might have to aim even beyond the fluent native speaker level. If communication with speakers in other communities is a goal, then different dialects might have to be taken into account. If literacy is the goal, then the types and amount of available literature would have to be considered.

All this brings us back to an earlier question, "Why ask the school to take on this task?". In many cases it might be very fruitful to ask if the school is the appropriate place for this kind of learning. If the program is initiated in the school, this situation could create a new need and perhaps desire for learning the language, but it would be unwise to count on that. The need and the factors that might affect the desire to learn the language would likely be present outside the school. But the facilities for practice might not exist there. Fishman, in his survey of bilingual schooling on an international scale (Fishman, 1974) has pointed out that school use of a language is just not enough--in other words, there must be support for and use of the language in the community in order for the school to be successful in using the language as a medium or subject of instruction. In cases that would be similar to Algonquian as a second language programs, he found that selectivity in the admission of students to the program and the status of the language in the community were most predictive of the success of the program. In programs similar to Algonquian as a medium of instruction situations, the major predictor was whether the language was that of formal institutions outside the school. It is difficult and expensive to set up most school language programs. Curriculum materials have to be developed and these often necessitate a lot of research if they are to be done properly. Appropriate staff has to be found or trained. In the long run it might be easier, cheaper and more effective if the community took steps to change the status of the Algonquian language or to alter the pattern of its use. In creating more need and desire for the language, the opportunity for practice might evolve naturally in the community.

Up to this point I have been discussing implications of two objectives for Algonquian language programs--objectives which are usually implicit in the existence and design of such programs. I turn now to some explicit objectives for the introduction of Indian languages into Indian schools. The National Indian Brotherhood strongly feels that Indian languages should have a place in all schools for Indian children. In Indian Control of Indian Education it states (page 15):

> The Indian people are expressing growing concern that the native languages are being lost; that the younger generation can no longer speak or understand their mother tongue. If the Indian identity is to be preserved, steps must be taken to reverse this trend. While much can be done by parents in the home and by the community on the reserve to foster facility in speaking and understanding, there is a great need for formal instruction in the language. There are two aspects to this language instruction: (1) teaching in the native language, and (2) teaching the native language.
>
> It is generally accepted that pre-school and primary school classes should be taught in the language of the community.

There are a number of objectives implied here. One is that schools for children who speak an Indian language and schools for those who do not should have Indian language programs. This would mean that the suggestions I have just made about sending the problem back to the community would not be acceptable. However, the changes that I proposed a community might make in the status and use of the language could be used in conjunction with a school program to improve the conditions for language learning. Indeed, the N.I.B. statement directly expresses this possibility. Another objective is that the trend of language shift away from the Indian languages be reversed. In order for this to be achieved either the school must create a new need and desire as well as provide the practice or the community must. An objective implied in the statement is that a sense of Indian identity should be instilled in the child through the learning of the language. Also implied here is that the value of having a sense of Indian identity and of being able to speak an Indian language is equally relevant to all Indian children. This is a logical variation on the question discussed earlier regarding the relationship between Indian identity and improved school achievement. The same caveat would apply, however. If the child finds the language learning distressing or meaningless, it may well be that the reverse of the desired objective may result. The statement gives no indication of the level of language facility it would aim at.

Very little is said in Indian Control of Indian Education about the N.I.B.'s goals for school achievement in the usual white sense. I infer from the statement that "pre-school and primary school classes should be taught in the language of the community" that at least part of the purpose would be to give any Indian child, particularly one with little or no command of the official language, a better chance at regular school achievement. In the light of my previous comments it would seem to be important that care be taken to select or evolve a format that is likely to achieve this end. If all or most school subjects are to be taught to Algonquian-speaking children through the medium of an Algonquian language, a gargantuan curriculum development task lies ahead One problem in this regard is to decide on the content and to make sure that it is authentic for the culture, the language and the school context. Another is to organize it in such a way that the later introduction of official language medium work will not provide a major conflict.

In conclusion, let me outline what I consider to be the crucial issues in all this for Indians, educators or academics involved in Algonquian school language programs. The first and overriding point is that objectives for any program must be carefully considered in the light of factual information about the community, the desired effects both within and outside of the school, and the likelihood that any language program or any particular language program could produce those effects. As a subsidiary to this main point, it is evident that existing language programs should be evaluated properly in terms of their objectives. Such evaluations could lead to effective adjustments in the objectives or in the programs themselves, and would provide much needed information to those working on similar programs. It is also apparent that the amount, type and function of the language to be used in the program must be decided on in relation to the objectives for the program. And finally, the relationship between the linguistic and cultural objectives of any program should be an important factor in decision making about the format and content of a language program.

All of this implies that there is a great deal of work to be done. Planners need to have accurate information about community aspirations, the role of the school in the community, and the structure and function of the language or languages of the community. Information about existing language programs should be collected and distributed to planners. The development of effective curriculum materials for first and second language programs necessitates a great deal of research. Existing materials must be evaluated for their potential usefulness in a variety of other programs. And suitable instructors must be found or trained. In the present climate of increased communication between Indians, educators and academics, I am hopeful that the resources of all three groups can be combined to give Algonquian languages a meaningful place in Indian education.

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# RETICENCE AND SELF-EXPRESSION: A STUDY OF STYLE IN SOCIAL RELATIONSHIPS

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Speaking of human nature (rather than of particular cultures), Simmel has claimed that the knowledge one gains from interpersonal relationships differs distinctively from all other kindsof knowledge because of man's unique potential for secrecy (1950:310). The ability to conceal oneself and thereby to regulate privacy, is developed conceptually by Murphy (1964). He clarifies Simmel's distinction between social distance vis-a-vis a stranger, and social distance vis-a-vis more intimate relationships. The stranger-relationship is threatening because of the lack of knowledge of the other, and secrecy, as a (largely reactive) means of controlling social distance, reduces this threat. In this paper I attempt to extend and modify the notion of secrecy, by depicting Cree reticence as more proactive, or positively directed by internal experience. Reticence, for instance, may serve a protective function until mutual knowledge allows more confident communications (although reticence is not the only possible response). At the other extreme, where mutual knowledge is well developed, interpersonal relationships are quite different, for the threat is no longer due to lack of knowledge of each other, but rather to the presence of this knowledge. Each individual's privacy is at stake, and now reticence allows appropriate disclosure, so that each individual may maintain his autonomy.

The form and content of the conceal/reveal balance for any individual varies according to his particular personality-in-culture attributes, and according to particular interaction situations. Murphy discusses the Tuareg as an illustration of his more general contention that "an area of privacy...is maintained by all, and reserve and restraint are common, though not constant, factors in all social relationships" (1964:1257). The possibility of many American Indian cultures sharing a common quality of restraint or reticence has been suggested by the Spindlers (1957:148), and developed ethnohistorically and psychoanalytically by Hallowell for the Northeastern Woodlands cultures as a whole (1955:132-150). Basso (1970) develops the use of silence as an aspect of Apache reticence. The concept of a typical Indian reticence in social relationships probably has some substantial reality at a cross-cultural level, but this concept has yet to be given adequate precision, and needs further specific, precise description and explanation from individual cultural settings.[2] This paper attempts, for an Eastern Cree community, such a description and explanation.

We describe Indians as reticent, finding that they tend to understate their emotions in their manifest behaviour. But if we pick up the stick of culture-perception at the other end, looking at our culture through Indian eyes, we will find that we mask our true feelings with a conspicuous flim-flam of over-expression; that we sometimes even confuse ourselves, as to what the real feelings are. By Eastern Cree standards, we desire and expect an unrealistic amount (and kind) of public emotional response and gratification. I believe that a Cree depiction of Whiteman personality would emphasize our need for a rather superficial and overt form of interpersonal affective reassurance.

In our own culture, drivenness and competition in a milieu of economic individualism (Henry 1966:13-44) produces a fear of exposure and, in consequence, an anxious concern, not only for affective reassurance, but also for privacy. But this privacy is maintained by more than walls and windowshades. It is maintained via a network of social artifice that is largely (but not entirely) unconsciously or semiconsciously learned, embodied, and taught. The habitual quality of this artificial style contributes, in turn, to a proclivity (both deliberate and unconscious) on the part of social scientists and others to assign a high value to such abstract explanations as game theory, models of pragmatic strategy and terms and concepts from the profession of acting. A sophisticated example is found in Goffman's The Presentation of Self in Everyday Life.

For the society he describes, Goffman may be phenomenlogically precise, but the cross-cultural relevance of his concepts is surely limited. Certainly his approach is apt for understanding how urban North Americans, for instance, nurture a cosmopolitan theme of social dilettantism. Not that our "typical personality" is simply and massively that of a homogeneous superorganic social dilettante, but that the theme of dilettantism is a highly congenial and commonly utilized relief, for many individuals, from the onerous pressures of individualistic competitivism. For many people, strategic role playing also is the pragmatic face of competitive interaction, (and the face of its inverse--the hedonic id-release world of sensate self-indulgence--the ultimate commodity). Perhaps the most conspicuous expression of this aspect as a valued quality lies in the way our culture idolizes entertainers and actors for their public display of psychosocial artifice and role playing. These highly paid and admired individuals allow their audience vicariously to participate in a wide variety of roles, dramatic and profound, or glamorous and superficial, or simply new and different.

The privacy, concealment, or social distance obtained in our culture through social artifice or dilettantism is skillfully used to advantage by anthropologists as well as others. Berreman (1962), leaning heavily on Goffman, gives us an excellent and explicit case of the successful use of this method-style. Yet even where the culture under study is concerned to present a "taxonomy" of roles (in Berreman's case a caste-structured society) "...it might have been better to have more Berreman and less Goffman." (Mintz 1963:1363).

But the Eastern Cree, whose style I seek to understand, achieve their social distance with minimal artifice, through a more direct (and consequently more personally honest) reticence. Distance is regulated by the extent to which a person expresses himself, rather than communicating a role, or disguised self.

In such a fundamentally different milieu, Goffman's categories or Berreman's methods are too out-of-context and superficial to be very useful. They could be applied, but the imposition seems inappropriate.

The translation of Cree concepts of proper behaviour into English, while far more appropriate, is a very difficult task, all the more so when some of the concepts remain implicit in the culture under study, embodied in action more than in language. Some of the concepts are also implicit in the ethnographer's data, to be extracted, only after probing speculation and extended reappraisal.

## RETICENCE AND SELF-EXPRESSION: THE STRANGER

Typically, the stranger is initially impressed by the apparent shyness or even apparent sulleness of many Cree. But further observation begins to uncover the inadequacy of these first impressions. A brief illustration from my own early field experience may clarify this point.

As we flew into Rupert's House for the first time, we were uncertain of what sort of reception to expect, as most strangers must be. The sketchy background offered in our reading and a few days in Moosonee had done little to lend confidence to our hopes for a friendly reception. I expected something on the order to "typical Indian reticence" or stoicism. I was surprised to see thirty or forty people come down to the plane as we taxied in. But my surprise was increased, and changed to a guarded uncertainty when I stepped from the plane to see that, in the sea of faces looking directly at me, no one was smiling. I could not be sure of what their faces expressed. As the summer progressed, I learned that the unreadable facial expression was not accidental. Neither was it simply shy or sullen. It was deliberately non-committal or unexpressive--reticent.

Towards the summer's end I discovered some of the functional attributes of the group of non-committal faces that meet incoming people. It is very like a projective test. The newcomer is quite literally faced with a situation that he cannot easily assess on the basis of cues in the situation itself, and he feels pressed to react--for all eyes are on him. His response, like the projective test response, serves to tell the group what kind of personality they are going to deal with, or avoid dealing with. The response to this test is usually quite revealing. I began to find the situation not only revealing, but also entertaining, and by the summer's end I was often a member of the group, and watched as impassively as I could each new stranger's discomfiture and striving for recovery. After one young hopeful fisherman had made a faltering and fading attempt to show a careless good will attitude to all, my interpreter, Willy Weistchee, commented, "Well, Dick, there's your replacement." When I asked if we had looked that green when we came in, Willy smiled a little and nodded.

Perhaps this is not a new situation. Delanglez cites the case of M. Couture, who in 1663 tried to get to James Bay from the south. He had reached Lake Nemiska, less than 100 miles from his destination, when:

> ...the Indians who were in said place, never having seen Europeans, were suspicious of them and gave signs of their dislike, so that he (Couture) felt bound not to go farther; and after trading with them and giving them presents in view of a future alliance he came back to Quebec (Delanglez 1948: 160, bracked insertion mine).

Couture apparently felt threatened by his reception, and chose to withdraw. Yet less than a decade later, Pere Albanel's group made the trip over the same route without becoming so discouraged. It is worth noting that Couture was almost certainly not really in danger, for my readings have yet to disclose the serious injury of a White man by an Indian[3] anywhere in the entire Rupert's House-Eastmain-Nemiska-Mistassini-Nichikun-Neoskweskau-Waswanipi area. The Hannah Bay massacre was done in desperation by Moose Indians (Hind 1863b:16-17 and Preston 1975:324-325; 1976:142-146), and this is the only such mayhem I have any record of. Had Couture persisted in his exploration, he might have claimed the area for France, beating Gillam's ship (carrying Grosseilliers to the establishment of Fort Charles and claiming the area for England) by five years. Couture's meeting with a group of reticent Indians was apprently, then, something of a historical moment of truth! Had he met this test with more equanimity, the history of James

Bay and Nouveau-Quebec might have been significantly different.[4]

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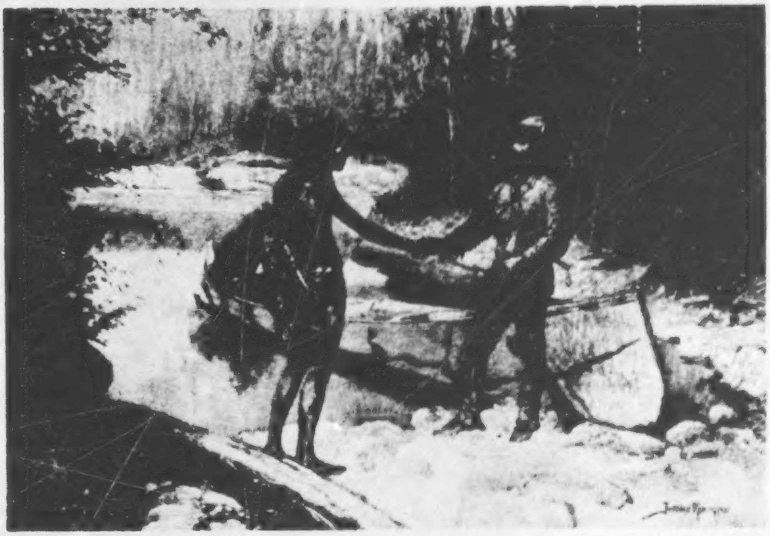

Frederick Remington, "The Coureur du Bois and the Savage"
From THE FREDERICK REMINGTON BOOK. Copyright 1966 by Harold McCracken. Published by Doubleday & Company, Inc.

Remington depicts the meeting of strangers that I describe on page 10, following, as a type (1) meeting. One can only speculate on its aptness to Couture's meeting.

Compare the posture of the men as expressions of personal-social style. The Indian is relaxed in body and facial expression, no hand on a weapon, and with his eyes meeting the stranger on a direct one-to-one, rather than superior-subordinate, basis. The Whiteman's elevated chin, body tension, with one hand on his weapon, expresses his need to compensate for personal uncertainty by appearing very sure of himself and superior in relation to the Indian. Remington's visual perceptiveness was based on first-hand experience of both cultures.

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My field experience and reading suggests that what Couture interpreted as hostility or sullennesss may well have been a misinterpretation of his situation. The Nemiska Indians, faced with a stranger, probably were assessing his trustworthiness as an individual before they would risk any interaction or cooperation. From the Indian's point of view, such a testing of the stranger was eminently sensible, since less critical acceptance of strangers might have proven a serious mistake.[5] But from the White man's point of view, reticence may be more unsettling than a manifest show of awe, respect, cheerful trust, or even of hostility and suspicion.

To return to the contemporary situation of meeting strangers, the interaction typically takes one of three directions. Since the arrivee cannot step back into the plane without appearing a fool and a coward, he makes a response that approximates one of the following types, or acted roles:

(1) The overt manifestation of an attitude of confident good will, more appropriate to urban Canada and America than to James Bay. This kind of self-assured presentation is the most common response observed. It is probably often sincerely motivated, but by Cree standards it is over-demonstrative and thus too contrived and self-assertive. So role behaviour that passes as appropriate to the status of the person in his modern urban social setting is rejected by the Indian observer as ostentatious, proud, and improper--even childish. I have seen a teasing re-enactment of such a Whiteman greeting performed with skillful satire, done not by a teenage boy (as we might expect) but by a bitter and drunken old man. His mimicry was received with reticence by the others, who found his effort too blatant to merit a more open and amused response.

(2) A loss of confidence on the part of the arrivee may be followed by withdrawal into an attitude of shyness or sullenness. This is a non-greeting, interpreted by the Indians as an unwillingness to meet and interact with them, due to fear or dislike or both.

(3) An entry with more equanimity and propriety may be observed with the arrival of an Indian or a White man who is familiar with the Indian standards of proper behaviour. Manifest emotion is at a minimum, but this also means no visible effort at secrecy or restraint. The arrivee is calm and relaxed, not indifferent to the situation but rather showing composure in

in meeting people on an individual-to-individual basis. He is not "faced down" by the group, but responds quietly. Where the persons have not seen each other for some time, a single, nearly limp handshake is appropriate often with the verbal greeting "Wachia" or "Bosure" or "Hello".[6] Apparently the Cree never had words corresponding to the English "hello" or "good-bye", until they borrowed European terms. A meeting or parting situation is, after all, self-evident, and the emotions involved are understood without need for these words. A less common but more expressive act of greeting or parting is kissing. I have seen this only a few times; each instance was at once unembarrassed and gentle, and not erotic.

As a final note on styles of greeting, I will describe a case of a non-greeting that serves to illustrate the contrast between White and Indian patterns of behaviour. The particular event involved two tourists who came for a few hours of picture-taking while the plane went up the coast and returned. Upon stepping from the plane, the two men responded to the group "test" by trying to act as if it hadn't happened and the Indians weren't noticeably there, a variation on the withdrawal response. They proceeded to a point in front of the Hudson's Bay store, where one took movies as the other strode confidently up to the door and opened it, as if he would go right in. The cameraman stopped filming at this point, whereupon the actor did an abrupt about-face and let the door shut. The two then remained outside the store with the obvious intention of filming emerging Indians. This action was observed by a number of Indians inside the store, including one man who had purchased a fifty pound bag of flour and was ready to leave, when he realized that he was expected to play the role of the white man's "Indian bearing his grub from the Hudson's Bay Company Post." He was not pleased by the prospect, and waited for several minutes. The tourists outside and the other Indians inside also waited. Then the man resigned himself to bearing the brunt of the tourists' bad manners, shouldered the flour and went out, not looking at the camera as he went by.

At this point, one of the more acculturated Indians inside the store decided to react with less than reticence. Improper behaviour is sometimes teased by the Cree with a mimicry of the offending act, or by a satirical re-enactment of a situation (as in the case of the drunken old man's clowning his way through a Whiteman greeting, cited above). In this case, using the flour-carrying as a vehicle to satirize the Whitemen's behaviour, this man also shouldered a sack of flour, went boldly out of the door (the cameraman rose to the bait, of course) and strode assertively straight up to the camera, waving his cap and looking right at the camera, as he went. At this point he did an abrupt about-face and walked calmly and quietly back into the store, set down the flour and went back to his business. No one laughed out loud, but everyone watched. The point of the satire was, for the most part, lost on the White men, who showed surprise and confusion; then moved on to photograph other things. This extended anecdote is more than wryly amusing. It is an excellent example of a discontinuity in what was communicated between Indian and Whiteman, and further serves to illustrate some standards of proper behaviour that fall in the category of self-expression. This example criticizes improper behaviour by means of deliberate, conspicuous action, where one acts with the realization that all eyes are on him. While he was expressing himself in the sense of acting according to his feelings about the Whiteman's behaviour, his actions did not so much express himself as they expressed, in mocking form, the expectations and behaviour of the Whitemen. It is consistent with Cree standards of proper behaviour that conspicuous acts should not be a case of self-assertion, but rather a teasing re-eanctment of someone else's foolishness. Again, direct mockery of this sort is not common, and the Indian involved was an unusually bold individual, by Rupert's House standards. Perhaps the conspicuous action was more appropriate to the teasing of strangers, by virtue of their more gross breach of proper behaviour. Also, perhaps it was an opportunity to express resentment towards unsocial Whiteman behaviour. Furthermore, such mimicry is obvious role-playing, and has a humorous or contrived purpose for the Cree.

## RETICENCE AND SELF-EXPRESSION: KNOWN PERSONS

The very high degree of intimacy that characterizes relationships with known persons means that teasing will be understood in all its subtlety, so that teasing need not be conspicuous to be understood. More typical is the indirect teasing rebuke; one that avoids a direct confrontation by means of physical and/or verbal restraint. The teasing may not be done eye-to-eye with the transgressor. Such action might be too much like a threat, and threats are common only towards children. Between adults, a threat is a very serious affair, where tension is brought into the situation and violence is a real and threatening possibility. More often, the tease will seemingly ignore the presence of the transgressor, or it may be made in his absence, to reach him by word of mouth, or not at all. Most individuals assume that an observed mistake will be teased, whether they witness the actual teasing or not. This expectation is an effective means to social control, and results in some definable types of behaviour that seek to avoid situations leading to observable mistakes or failures.

(1) Most common is the type of behaviour that appears passive or negative, where a person does not respond to a challenge, request, or opportunity (contributing to a "lazy Indian" stereotype). Where the individual sees failure as a likely possibility (or success as demeaning), he will probably not try it at all, thus avoiding the expected consequences of

observed incompetence; ridicule through teasing or satire. Failure might arise through actual inability to handle a task, but often may result from ambiguity or uncertainty regarding the nature of the task, or the means for achieving the task.

(2) When a person does attempt a task, he is likely to stick with it with a grim determination (contributing to a "stoical superman" stereotype). This is apparently a widespread pattern in both time and space. LeClercq writes of the Micmac, in 1691:

> They do not know what it is, as a rule, to give up an enterprise which they once have formed, especially if it is public and known to their fellow-countrymen; for they fear to incur the reproach that would be made to them that they did not have heart enough to carry out the design (1910:245).

LeClercq's use of the phrase "have heart enough", combining will and stamina and determination, leads to an important concept in describing interpersonal style in Cree culture. Probably the closest English concept is that of competence. Competence refers to behaviour that demonstrates "effective interaction with the environment" (White 1959:317) and to a synthesis of internal strength or will ("heart") and aptitude that is the potential for such behaviour. A man's competence is measured by what he does with what he has. Bush living was (and to an extent still is) characterized by a demand for high competence using very little to work with; a man had to do a great deal with very little social or technological aid, simply to maintain his family. Sometimes competence was not enough, then an element of fortune or luck took a crucial part in the individual's life. The Cree /tsed-tchi-tik/ translates as "in hope that" and is the closest approximation I have obtained to the English "luck". Luck, as seen by the Cree individual in the sense of hoping (that he will be capable of meeting a task), describes (roughly) another important Cree native concept, and the concern with luck in ritual behaviour is an important facet of self-expression. The anxiety that accompanies dependence on luck may also partially explain the role of reticence in some aspects of Cree culture (e.g., meeting strangers).

To return to the concept of competence, Colson has developed the concept with a focus on "...ability to cope with men and affairs." (1967:109) She extends the meaning beyond the purely technical sense by including political behaviour. For the Eastern Cree, however, I believe that the concept should be broadened further to include social and mental capability (whether consciously or unconsciously patterned). A more precise definition should combine competence, with its essentially practical or technical overtones, with a more personal or psychosocial kind of competence, implied by LeClercq's term heart (the Cree term eh-suyts-de-het). In my own field experience, one bilingual informant used heart to explain an unusually aggressive child. "Kids who fight a lot have a bad heart. One kid in that house is always fighting. He has a bad heart." I suspect that the use of this term by these two diverse persons (LeClercq and my Cree informant) is not entirely fortuitous; having a good heart may have similar meaning in both cultures, so that the English word was easily integrated with the Cree concept. Crow and Tewa "heart" concepts for instance, also imply self-control and fortitude (Lowie 1936: 196; Dozier 1966:68). The general meaning in both contexts is close to what we sometimes call spirit or will, or personality strength. While the Cree is typically a very practical person in many ways, he is also very deeply aware of the personal side of life, to social and mental competence. I am describing psychological qualities that are difficult to define with precision. Through a synthesis of high sensitivity and a personality structure well integrated with their cultural context, most of the Cree so internalize the values of their culture that self-control is an adequate and congenial means of coping with the contingencies of both the physical and social environment. High sensitivity is mentioned in a more narrow context by Anastasi (1935, cited in Hallowell 1955:130) when she speaks of the hunter's skill in responding to very slight sensory cues. For the Cree I believe the high cue sensitivity extends to social interaction. The shift from hunting and trapping cues to social cues is not a forced analogy on my part, for trapping and hunting require more than a cue sensitivity to the location of an animal. The hunter must know the animal, his condition and mood, and the possible avenues of response that the animal may choose when it is aware of the hunter or the trap. In effect, man and animal interact; knowing the strengths and attitudes of the animal is the basis of the hunter's or trapper's strategy. A similar sensitivity to social cues is the source of the skill, precision, and subtlety that characterizes Cree human relations. The subarctic environment challenges the Cree hunter with a difficult terrain and climate, plus a sparse animal population. The hunter's world is characterized by the contingencies of an ungenerous and inconsistent environment. But the social environment is also one of contingencies. Here, as in hunting, the individual depends on his own and on others' competence.

I cannot find an English term that gives *precise* connotations to all aspects of competence, technical, social, and mental. The Cree naš-ekstat is used to apply to a *similar* range of competence. Where the action involves a competent hunter, naš-ekstat would translate as "very capable" (technical competence) at hunting. When applied to thinking (mental) competence, naš-ekstat would translate as "very smart." For a chief who is noted for his oratory, naš-ekstat would translate as "very good" at speech making (social competence). Competence, in the special broad sense outlined above, is very much involved with the standards for reticence and self-expression, and we will build on this base in further defining reticence and its role in mediating self-expression. Reticence is here defined as that area of self-control that directly affects personal exposure or self-expression. The degree of self-control increases as behaviour is characterized by its "quietness", "shyness", "withdrawal", and "avoidance." Several informants have described individuals as quiet, in a way that implied their approval of this behaviour. But a quiet man need not by shy. He may be confident of himself, he may be aware of, and active in, a situation. His quiet quality is not a withdrawal from the situation, it is rather a capable participation, without showing himself assertively in his participation.

Eddie Esau, a Rupert's House Indian living at Moose Factory, was approached one night by two young members of an Indian gang with a reputation for urban hoodlum behaviour. One of the boys pulled out a knife and told Eddie that he was going to stick him with it. Eddie reacted with reticence and competence. He stood his ground, said nothing, and grabbed the attacking boy's arm, twisted it, and took the knife. He then threw the knife away into tall grass where it was not likely to be found, and walked away from his assailants. He did not act aggressively by attacking them in return, nor did he interfere in the situation any further than he was forced to for his own safety. Reporting the incident (or "taking in" the assailants) to the police might seem appropriate to a Euro-canadian, but not the Eastern Cree. Such an act would be below his standard of social competence, and would carry implications of interference with the others, who were less socially competent than Eddie and had just been reminded of it. And even worse, he would have allowed himself to become involved in aggressive (retaliatory) behaviour.

While quiet competence is viewed as ideal, it is clearly differentiated from other 'quiet' categories. Shy behaviour in social settings is interpreted as passive or even negative rejection of the other persons involved--as a disinclination to interact even on a quiet basis. By Cree standards, what we call either "quiet" or "shy" is normal, what we call withdrawn would be called "shy" /esagwemut/ by the Cree. I do not know of a Cree term for "withdrawn", but it implies an active disinclination to interact, or a communication that personal secrecy is preferred.

These distinctions (quiet, shy, withdrawn) may be found in the use of humour in its less dramatic forms. Reticence does not lead to a joyless or humourless outlook on life. On the contrary, self-expression in personal warmth and humour is very much realized and enjoyed. The French joie de vivre is, in some ways, similar to the congenial social reciprocity of the Cree, but French exhilaration is manifested through extroverted, other-directed expression. Cree feelings are more private and internal; yet no less shared and mutually understood.

Humour also serves the milder functions of expressing (and avoiding) embarrassment, and humour in response to incompetent behaviour is a way of showing that the embarrassed person need not feel too concerned about it. Also, as mentioned earlier, humour may be teasing or mocking in intent. The point here is that the shy person probably will rarely use humour, while the quiet person will make frequent use of mild forms. A useful example is the story of a young man who did not know his exact age. One day a group of the young men were comparing their ages. When he was asked, he replied, "I guess I'm old enough to be a man, because I'm too big to be a pup." From the Cree point of view, this story is a case where a clever man turns a potentially embarrassing question into a funny (for the Cree) situation. For a quietly restrained form of self-expression, this was the appropriate (socially competent) reply, because it demonstrates his maturity at the same time that he was claiming the maturity. If he had been shy (less socially competent) he might have simply replied, "I don't know." If he had chosen to withdraw, he would have not replied at all. An aggressive, defensive, reply like, "It's none of your business," would be almost unthinkable.

I have used humour to illustrate contrasts between quiet, shy and withdrawn behaviours. With respect to avoidance behaviour, ritual forms that are common to American Indian cultures (mother-in-law/ son-in-law; father-in-law/ daughter-in-law) occur also at Rupert House where the daughter or son-in-law (ideally) does not initiate conversation with her/his in-laws, yet enters freely into the interaction once the elder-in-law has begun. At Fort George, (200 miles north of

Rupert House) conversation with in-laws may (ideally) take place _only_ through the use of an intermediary.[7] Differing from these _respect_ situations, Cree avoidance behaviour is construed as an unspoken but real manifestation of strong dislike or anger or both, depending on the situation. Avoidance in this cultural setting could not be neutral or respectful of another's status (or a mask to hide feelings of hostility, jealousy or lust). It is, rather, a refusal to seek or accept social contact, due either to the anticipated incompetence of others, or to one's own incompetence.

In sum, the kind of reticence I have defined as _quiet_ behaviour serves to define the range of self-expression that is regarded as proper. Behaviour that I have classed as shyness, withdrawal, or avoidance involves types of reticence that block self-expression more than channel it, and are usually not acceptable forms. They are unacceptable because of their lack of a contribution to social composure, similar to the way that a presentation of role personalities or partial, contrived selves unacceptably complicates social composure.

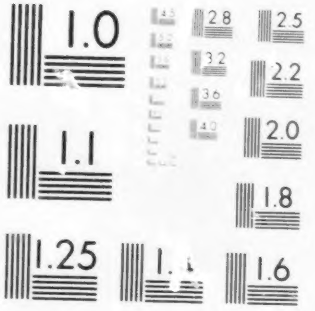
I do not deny the existence of any status-role differentiation, but I seek to emphasize the minimizing of a multifaceted self-presentation with its goal of effective image promotion. The whole question of "masking" and selective self-exposure, and the Cree alternative of personal honesty mediated by reticence, stems from the universal human awareness that other persons may seek to deceive or conceal in their social relationships. Ulterior motives are a chronic possibility in any culture, and the Cree have placed a high value upon personal honesty. While this value was interpreted by some early writers as the essential virtue of simple children of nature, some writers also noted that should a trader attempt to take gross advantage of an exchange, the patterns of direct honesty gave way to very clever deception. That is, the honesty patterns did not exist due to the naivete of the Indians, but rather due to their sophisticated preference for social interaction on the basis of mutual honesty. Cree individuals find that lifelong social interaction among the same small group is best managed where each avoids creating stress: (1) through not interfering with others and, (2) through the maintenance of consistent personal holism and honesty, mediated by reticence but not by contrivance and the presentation of many "selves." By comparison with the Cree, our management of many roles through the presentation of many selves imposes a stress load on our social relations that, in its extreme cases, renders personality control so complex that the tenuous interrelationships between selves may lead to a loss of contact with socio-psychological reality--to an overvaluing of fantasy and to some degree of personal disintegration.

For the Cree, one holistic self per individual is preferred, and alternate facets or self-images are not acceptable, but rather considered to be contrived masks that at least make dependable social interaction complex and stressful, and if extreme, create a threat to social interaction. For example, masking is associated with sorcery, where the sorcerer is careful to mask his feelings and intentions. But even here, the sorcerer hides behind a contrived mask of normalcy. So far as I know, the presentation of a contrived self or mask is limited to the use of humorous mimicry at one extreme, and the special case of the concealment of enmity and/or sorcery, at the other extreme. Anything beyond obvious and humorous mimicry is regarded as deceit, at least I assess the response to such behaviour on my part and the part of other outsiders in this way. I have not seen evidence of such behaviour by the Cree themselves. Perhaps it is appropriate to mention that there are no Cree masks (in the sense of the ceremonial masks of the Iroquois, the Pueblo Indians, the Northwest coast tribes, some Eskimos, etc.) used, nor have I seen any pictorial representation of a face, except by school children. While the Eastern Cree apparently made no pictographs like the Ojibwa did, they did decorate clothing, tent flaps, and other artifacts, but faces are not found in this decoration.

As an observation on the relation between culture and mental health, I suggest that reticence is not simply a defense mechanism; it has a positive, facilitating function and meaning in this cultural milieu. Perhaps the implicit tendency of psychological explanation to focus on personal pathology is most unfortunate in contexts like this. In our own culture, for instance, the mask of the capable actor provides the audience with a selected sample of the personality of postulated people in postulated situations, offering insight into human qualities that may not be easily obtained in ordinary social interaction. Masking in this situation is not a defense but a creative device.

In Cree culture, the control of self-expression that is obtained through reticence not only defends by concealing, it also focuses perceptions on a limited but direct expression of the personality. This focus allows a measured sensitivity and intensity of interpersonal perception that accumulates to a depth of understanding and interpersonal sensitivity, resembling the abilities of a psychiatrist. The development of this sensitivity leads to self-knowledge and self-awareness, as well as the awareness and knowledge of other selves. But primacy is given to personal knowledge and self-awareness, rather than to external knowledge.

Goffman describes a style of social relationships that is "other-directed", where each individual seeks to establish manipulative control over others (see also Thibaut and Kelley 1959:100ff.). The Eastern Cree, on the other hand, are more self-directed. The self is not presented with a selection from an elaborate supplementary framework of social artifice, but rather maintains an internal preference for direct and honest self-expression, mediated, for the sake of privacy, by an aperature-like , single control of reticence. One does not present himself to manipulatively control others so much as he decides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MICROCOPY RESOLUTION TEST CHART
NATIONAL BUREAU OF STANDARDS 1963-A

to risk some exposure, controlling any tendency to self-manipulation, in the process of expressing himself.

The degree of normative compulsiveness involved in reticence in self-expression is relatively great, so that few types of assertive (non-reticent) behaviour are acceptable. Yet not all forms of expression must involve assertion. Besides the mild humour already mentioned, one of the most important modes of social competence is in the telling of stories, or narration.

Narration may refer in its content to almost any conceivable (to a Cree) subject, ranging from "myth" to current local events. It is a performing art, where the content must be scrupulously precise, but where individual styles are open to interpretation and self-expression. Narration may be used casually, in the conversation of two people, or more formally, when one narrator speaks to a group. One never interrupts (this would be imposing an interfering restraint on the narrator's self-expression) and even laughter is usually restrained until the narration is finished. Then people will go over the story to enjoy parts of it in a more unrestrained fashion, laughing at the funny parts and discussing the credibility of doubtful parts. Children play at narrating, and adults laugh when they hear the other children interrupt the narrator to correct a mistake. The adults' laughter teases the assertiveness of the children who are interrupting the narrator. The freedom from interruption has consequences for accurate persistence, since one may more easily or inadvertently modify a communication where interruption is occurring or anticipated, than where one does not compete for the attention of the audience. The narrator is aware that the audience will listen to the whole story as it is known by this particular person, and then may question or cirticize, perhaps expressing a concern for the precision, and rarely expressing concern over, say, dramatic impact. Reticence is evidenced in narrations by the importance of maintaining uninterrupted listening. But more than this, the form that a narration takes ensures that the events and processes making up the content of the narrative are also so rendered as to maintain their internal integrity. That is, each act within the narrative is reproduced with maximal precision (requiring controlled communication of precise detail).[8] Persons in the narratives often "speak" in the first person, and much of the social, physical and mental milieu is so well communicated by the directness, detail, and integration of the context that substantial portions are unambiguously understood without being rendered explicitly. A detailed treatment of Cree narration is available (Preston 1976).

Other modes of self-expression within a context of social competence include feasting, dancing, and more casual, or more intimate social interaction. In each case, the individual's competence is probably measured by a combination of his skill and capacity or stamina, although excessive or conspicuous behaviour would be ridiculed. Very little is known on these topics, but the amount of anthropological understanding is certainly not a measure of their importance in the culture.

Finally, there is another aspect of social competence, related to narration, but more broad and diffuse in scope. I refer to style in casual conversation. This apparently trivial matter is actually a very basic, distinctively defined, and subtly elaborated part of Cree social competence. The norm is again one of quiet self-expression; humour and teasing, and testing others through measured restraint. The subtlety (but not devious complexity) is very hard for a non-Indian to grasp, yet it is crucial to effective ethnography. An innocent sounding question or comment may be a restrained request or an invitation, and the failure to respond correctly may be a rebuke or an insult. As another example, a comment on some obvious fact (like the weather) may test the other's willingness to engage in further conversation, assure him of the speaker's mood, and perhaps more.

The ethnographer is at a particular disadvantage in trying to assess this kind of expression, since most of his observations will be of casual conversation either directed at him or made with the conscious awareness of his presence. He thereby structures the style and content of what people are likely to say. The fact that his data are taken in English rather than Cree is another contaminating factor. Still, he must make do with these contaminated data until he is well enough known to be largely ignored, and can record his data in Cree.

One of my first successful attempts at self-expression with appropriate reticence was the purchase of a house. Although the example is a casual one, it serves as a partial illustration of the style. I was visiting with Stuart in his new house, and we were comparing the new to the old in terms of the additional space he now enjoyed. After commenting on the inadequate size of the old house, he mentioned that the priest had offered him $75 for it, but he had not sold it since he felt it was worth $100 anyway. In effect, he was telling me what had happened in such a way that I might get the idea that I could buy it for $100, but his offer was indirect enough so that I could fail to acknowledge his point without blatantly refusing his house or his personal commitment in making his offer. I replied that I also thought it was worth $100, and that if no one else wanted it before April (this was January), I would have the money to buy it then myself. This was an adequately restrained way of saying that I wanted to buy the house and would be happy to pay the price that he asked but left him an 'out', if he did not want to commit himself to a sale to me, at the time. I think he was a little surprised that I had read the cues and responded correctly. He replied that he didn't think that anyone else was going to want it. That closed the bargain.

This example only begins to convey the subtle richness of the style of casual interpersonal relationships. Note that at no point was deception or devious side tracking used, but rather the hinting of limited but precise and honest disclosure. One of the important problems in this context is to discover how much this style of expression and communication is a matter of unthinking habit, and how much is deliberately and carefully planned. That is, to what extent is this behaviour unconsciously patterned? I believe that much of it is smoothly and carefully, yet casually, conscious in the minds of the users. I compare this style in Cree conversation to a kind of verbal chess game. Perhaps the conversation of a clever salesman is similar, but its premediated indirection and deceptive form, make it a chess game with devious content and rules. The salesman plays his verbal chess game to win by the deft use of artifice, where the Cree plays the game with honest rules with the purpose of keeping the game going. Winning or losing at Cree social interaction would be a sign that someone went wrong.

## RETICENCE AND SELF-EXPRESSION: VARIABILITY

To round out and clarify the picture presented above, I will discuss degrees of difference found in the expression of social competence in terms of: (1) the compulsiveness of particular social norms; (2) inter-community differences; (3) the behaviour of different individuals, and (4) intra-individual behaviour.

Culture is too complex and dynamic to be taken simply in terms of 'type' or 'modal' definitions of patterns or norms.

As Sapir has expressed it, understanding the reality and meaning of a cultural pattern demands an analysis of variation (1949:476). This dictum is here applied in several ways, including:

1. Varying normative compulsiveness, as illustrated by our loosely compulsive norm of "Christian behaviour" contrasted with the rigidly compulsive norm involved in our incest taboo.

2. Inter-community variance, that is, sub-culture variance.

3. Inter-individual variation within a culture, as discussed by Radin (1937) in his explanation of differential religious fervour.

4. Intra-individual variation over time, as each individual's own situation changes, both in terms of different daily events and in terms of the effects of continuous socialization, through the progressive stages of the life cycle.

These four dimensions--normative compulsiveness, inter-community variance, and inter- and intra-individual variance --are not isolable patterns in themselves, but rather four aspects of any culture pattern. Each dimension may be used as seems indicated by the nature of the data or by the interest of the analyst. In the following presentation, all four dimensions will be illustrated with the intention of increasing the precision and completeness of the explanation.

With respect to normative compulsiveness, I have already suggested that the norm of reticence in self-expression is relatively compulsive. The vagueness of this statement must be pinned down by answering the question, "Relative to what?" Two possibilities exist: relate the norm's degree of compulsiveness to other norms (1) in Cree culture, or (2) in the culture of the anthropologist. Many Cree norms are similar in their coercive strength to the norm for reticence in self-expression. Examples include norms for personal modesty, non-violence towards other people, food sharing where there is need, abhorrence of suicide or cannibalism, and observance of some basic kinds of hunting ritual. Each of these norms is very compulsive; there is very little room for deviance, and deviance would be expected to meet with strong negative opinions by most members of the group.

Less compulsive norms include child-rearing patterns, chastity, and hunting ritual that is not directed to the major forms of game. It is fair to say that Cree culture is characterized by many tight or compulsive norms, and that among these tight norms is the use of reticence in self-expression. A significant relaxation of the norm in less 'intimate' settings occurs during the celebrations such as the New Year, when everyone is expected to visit all the homes and a festive mood is more openly expressed. Periodic feasts and dances, and drinking behaviour or Pentacostal worship services (Preston 1975a) also fit this relaxation of the norm.

Inter-community (sub culture) variance will be only indicated, for lack of detailed study. For instance, in Rupert

House, the community where I have worked most extensively, the frustration of rapid and persistent acculturation has contributed, for some individuals, to a reinterpretation of traditional patterns of reticence into a mixture of resentment and suspicion and reticence, and to avoidance of social relations with some Eurocanadians. Another community, Fort George, had, until the late 1960's, a more specialized history of Eurocanadian contact, focussed by very effective missionizing to reinterpret reticence with a more open (Christian?) expression of smiling courtesy that approaches servile deference in some individuals. With the sudden rush of stranger Eurocanadians that the James Bay Hydro project brought, Fort George reticence has rapidly reinterpreted acceptable relationships with strangers; avoidance has approached and bypassed that at Rupert House. In both above-mentioned cases the reticence pattern is by not substantially displaced, but rather modified by adaptation to the colonial period of trader and missionary influence, and modified differently by the press of modern Eurocanadian influence.

Inter-individual variation is related to compulsive norms, in that we would not expect to find a wide range of individual variation. In fact, my data bear this expectation out. An example of maximal variance from the norm occurs in the behaviour of a chief. He has been called "the shouting chief" in response to his occasional loud pronouncements or orations. While these almost always express his status as as chief, he was noted for this quality before his election, so that the behaviour is not simply an expression of his being chief. An important qualifier is that he is not blatantly assertive in this behaviour, but rather expresses the norms of tact and skill, the social competence associated with quiet behaviour. While the chief varies from the norm of reticence by being less quiet, he nevertheless maintains with exemplary skill the other standards of humour and teasing and social restraint, and varies from the norm only in this limited way. His oratory is very skillful and enjoyed by the listeners. Differently variant, but notable, are a few people who have been steadily employed in association with White men. They show somewhat less reticence towards Whites and towards other Indians, joking and showing a relatively easy manner, no doubt facilitated by their bilingual abilities. Much less variance from the norm is shown by almost all the rest of the adult population. A few individuals vary in the direction of shy or even withdrawal behaviour.

Finally, there are some young adults, mostly ex-residential high school students, who show less reticence in some ways, often with a conscious effort to do so. Negative opinions fall heaviest on this group, though it is almost all expressed with restraint and fails to impress some of the youths enough to make them conform. At its maximum, however, the variance is neither great nor often threatening to the social stability. The norm remains a viable one, and includes some built-in flexibility as well. Particularly noticeable in a culture contact context, reticence is often reinterpreted by the Cree to a selective avoidance of people who do not respond adequately to proper social behaviour (lack the social competence to respond to cues).

As mentioned above in the outline of inter-community variance, further reinterpretation may develop into resentment and/or hostility (a correlate of apathy and dependence on a welfare economy). The precipitating factor is probably the loss of personal pride. The role of pride is crucial yet obscure, for any assertion of egoism is negatively sanctioned. Pride is covert, yet very much present, for a response to hurt pride is rather easily seen. This is probably because pride is closely tied to high degree of interpersonal sensitivity. Self-concealment and self-control is not managed by means of insensitivity to the emotional side of interpersonal relationships. Perhaps the high sensitivity of the Cree is managed and facilitated by the direct and measured way in which it is handled, expressed honestly and simply, but with restraint. The importance of personal pride serves the functional role of social control and cultural uniformity where individuals take pride in their adequate demonstration of personal competence (Preston 1970).

Intra-individual variance may be approached in two ways: (1) in terms of variance patterns within an abstracted typical life cycle, or (2) in terms of specific individuals over time. The latter approach may also be taken over a relatively short time (e.g. daily routine) or a life cycle. I will illustrate these approaches below. To go beyond the use of illustrations that I feel are particularly appropriate, to a truly thorough study of these aspects of variance, would be a major study in itself.

With respect to a typical life cycle, reticent norms begin to show at a very early age (easily discerned by age two), but they are frequently violated. Adults may laugh and even encourage a child who is assertive, so long as he is not violent or destructive. The approving comment, "He doesn't care what he does, that boy," refers to a boy who is socially assertive, but not to one who is destructively aggressive. The latter behaviour occurs when children victimize smaller playmates or when they are destructive of the property of others, and it draws harsh words or forcible removal by the child's parent. If the offending child's parent is not present, however, other adults will avoid interfering unless it is absolutely necessary. The absent parent is made aware of his child's transgressions by reports from other children or by adult gossip. Reticence grows fastest with girls, although they remain relatively unrestrained with small groups of age-mates of the same sex, and in early teenage may be quite rough and reckless. In general, internalization of the reticence norm increases slowly and gradually until marriage, when peer group relationships (for both sexes) are substantially and suddenly reduced. The individual shifts the focus of his social interactions to his family group, and exhibits a marked increase in reticence in his non-family relationships. This focus is maintained during the sexually productive life of the individual, and only in old age does reticence loosen up significantly. The pattern outlined above is subject to inter-personal variation as has been illustrated above.

Turning from the abstracted typical individual life cycle to actual, specific individual variance, we have noted that the chief shows variance in his reticence as his daily situation changes. When he wishes to make a point to a group, he may speak dramatically. On the other hand, he may use more typical reticence, as in a case of talking to one old woman in a large group. The whole group was his (intended and actual) audience, but he directed his narration to the one individual, relating with quiet style and humour how he had distributed parts of a sturgeon to various White men in the community and wound up with very little to eat for himself. Depending on his purpose and mood, then, he may vary from the norm or conform closely to it.

Finally, the variance shown by specific individuals during their life cycles reveals attributes of how the norm actually works within the developing personality of specific individuals. For example, Willy Weistchee, who acted as interpreter, informant and general manager during my first periods of field work, had internalized the normative standards. But his extended hospitalization between the ages of eleven and sixteen resulted in the internalization of new norms as well as a new language. He was bi-cultural as well as bilingual, which gave him a very complex role in Rupert's House. The fact that he could, and did, vary from the Cree norms had a nearly ostracizing effect (compounded by a physical disability that rendered him a partial cripple). That is, one must not only demonstrate mastery of the proper standards of conduct in social relationships. He must hold to these standards or else face the negative opinion that result from his transgressions. His high intelligence and well-developed (Cree) sense of self-control was expressed to me in advice ("Don't get excited." "Never lose your nerve.") and it served to his advantage in his adaptation to his difficult status. His premature death leaves unanswered the problem of how he would have resolved his conflicting standards for social relationships. Essentially the same problem is being faced by increasing numbers of children who go out to high school and then return to live in the home community. Here the focus shifts from conservative competence in traditional skills, behaviour, and knowledge to an acculturative competence along the lines developed by Colson, including the shift to "a self-confident willingness to experiment and an ability to mobilize their forces for successful learning." (1967:109)

A last example deals with an individual who has experienced relatively little contact with Eurocanadian culture, speaking no English or French and maintaining bush life activities as much as old age allows. John Blackned has

## NOTES

[1] Research in the Rupert's House, P.Q. vicinity was supported in 1963 and 1964 by a NIMH training grant, and in 1965-69 by grants from the National Museum of Man, Ottawa.

My approach to the problem follows guidelines given by Sapir for a more personalized approach to ethnography, outlined in some detail in an earlier paper (Preston 1966). Fieldwork with the Eastern Cree of Rupert's House, James Bay, Quebec, has been conducted over a period of twelve years (and continues) with about fourteen months' field work to date (Preston 1976:3-24).

[2] Wax and Wax refer to a closely related concept, an "ethic of non-interference" (1965:1079) that seeks to characterize many AmerInd cultures. Others might wish to extend these concepts to most small-scale (Wilson and Wilson 1945) or folk (Redfield 1953) cultures. Such correlations of ethic and scale are only suggestive until the concepts are defined with more precision and until the range of variance is defined for the cultures involved.

[3] A record not always reciprocated by White men, unfortunately.

[4] In fairness to Couture and to history, his record of the trip states that his Indian guides refused to go on down the river from Nemiska, and that this factor plus Couture's assessment of the attitudes of the Nemiska Indians influenced his decision to return to Quebec.

In fairness to the Indians involved and to history, Couture may well have put the onus for his failure on the Indians, since they were not likely to publish a rebuttal, and also were able to converse easily with the Nemiska Indians regarding the route (there was very little dialect difference). Delanglez cites the case of at least one Indian making the trip to Quebec from James Bay. This suggests the absence of animosity between local groups of Indians in this area (a conclusion that corresponds with all the evidence regarding the Cree-Montagnais-Naskapi inter-group relationships and the many long trips made by Indians throughout this area). The bulk of the indirect evidence, then, suggests that the Indians were not responsible for the failure of the expedition.

[5] In a personal communication, Honigmann has expressed his concern regarding proof of persistence over time and space. He feels that I, following Hallowell, assume substantial persistence on the basis of minimal documentation. I confess

myself guilty to this extent: from my readings in historical documents and from comparisons of patterns of persistence in folklore and language, and from a feel or intuition about the overall configuration of culture for this area, I feel confident enough to argue the probability of such persistence. Clearly I am not generalizing from adequate data, yet my grasp of the general picture leads me to believe, for now, in great persistence over time and space for much of Eastern Cree mental culture.

[6] The alternative forms here express about the same meaning. Wachia is a modification of "What Cheer" and Boshure is a modified "Bon Jour". The meaning to the Indians is not the original one, but something on the order of "shake hands," a request which applies to farewells just as much as to greetings. Implied in the meaning of "shake hands" is a direct person-to-person acknowledgment of good will and respect. Aboriginal patterns of meeting and leaving were probably quite different. "Hello" is appropriate where the person greeted is highly acculturated, for example, white-status individuals, or, when children try to get a response from a Eurocanadian visitor.

[7] This pattern is now diminished, as values associated with modernization diminish the respect shown for elders.

[8] Similar to the precise detail that the hunter obtains by his high sensitivity to cues, mentioned earlier.

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# DAWNLAND DIRECTORS: STATUS AND ROLE OF 17TH CENTURY WABANAKI SAGAMORES

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# DAWNLAND DIRECTORS:
# STATUS AND ROLE OF
# SEVENTEENTH-CENTURY
# WABANAKI SAGAMORES

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## *ABSTRACT*

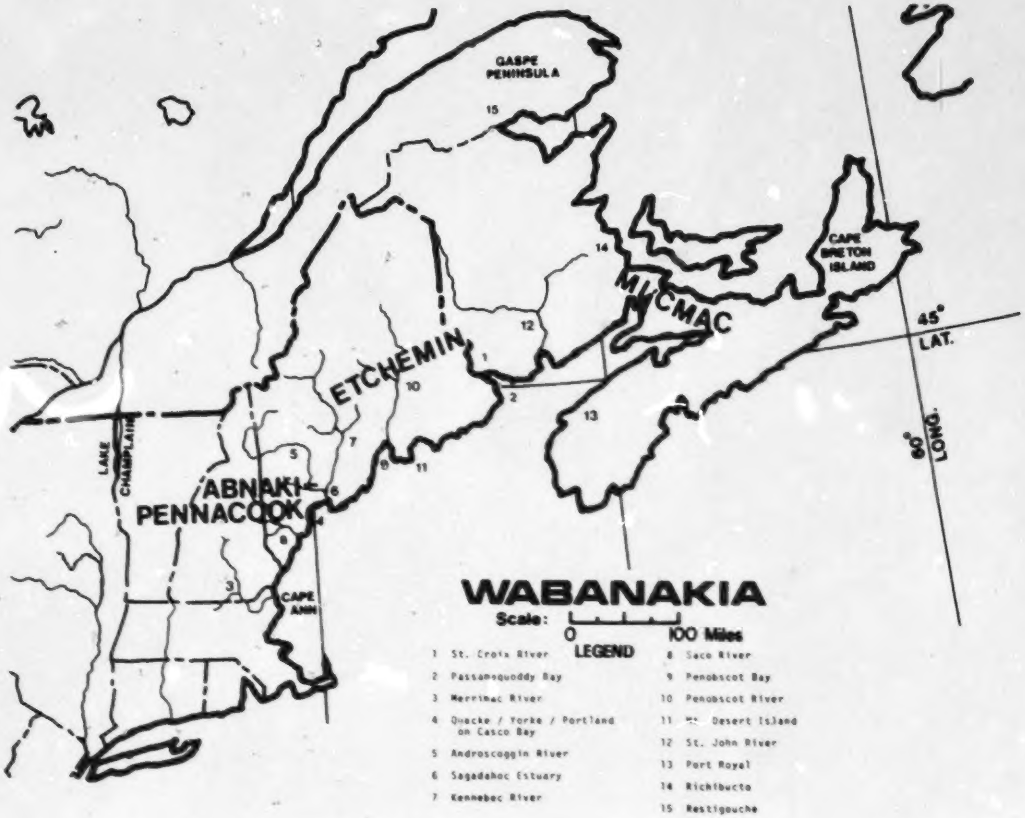
Sagamores were the sociopolitical leaders of the Dawnlanders–the Wabanaki Algonkian peoples of sub-St. Lawrence Canada and northern New England. This paper is a pioneer attempt to analyze ethnohistorical accounts of the Wabanaki with a theoretical model eclectically built from sociocultural anthropology to summarize meaningfully the status and role of Dawnland sagamores in the Early Historic Period (the 1600s). Both the topic and the method are believed to be new in Wabanaki studies to date.[1]

### THE WABANAKI OF THE NORTH-ATLANTIC SLOPE CULTURE AREA

(HUNTING-FISHING-GATHERING BASIC; MAIZE HORTICULTURE SUBSIDIARY WHERE PRESENT)

| (17) 17TH - CENTURY PEOPLES | (A) EXTRAORDINARY ALLIANCES | | (L) LANGUAGES & DIALECTS | (20) 20TH - CENTURY "TRIBES" |
|---|---|---|---|---|
| ① MICMAC (A CONFEDERATION) | (1B) MICMAC ALLIANCE (EACH ONE A SEPARATE SUPERCONFEDERATION) (OF 16TH-CENTURY NATIVE ORIGINS) | ② WABANAKI CONFEDERACY (A SUPER-SUPERCONFEDERATION) (OF 18TH-CENTURY FRENCH ORIGIN) | ① MICMAC | ① MICMAC |
| ② ETCHEMIN EASTERN | | | ② (2A) MALISEET- | ② MALISEET |
| ② ETCHEMIN WESTERN (EACH PART A SEPARATE CONFEDERATION) | (1A) ABENAKI CONFEDERACY | | ② (2B) PASSAMAQUODDY | ③ PASSAMAQUODDY |
| ③ (3A) ABNAKI- | | | ③ (3A) PENOBSCOT- | ④ PENOBSCOT |
| ③ (3B) PENNACOOK* (EACH ONE A SEPARATE CONFEDERATION) | | | ③ (3B) ABNAKI- | ⑤ ABENAKI |
| | | PLUS OTHER PEOPLES | ③ (3C) PENNACOOK* | |

*NOT ALL OF THE PEOPLES OF THE PENNACOOK CONFEDERACY ARE MEANT TO BE INCLUDED IN EITHER ALLIANCES (1A) & ② OR IN LANGUAGE ③

GASPE PENINSULA
CAPE BRETON ISLAND
45° LAT.
60° LONG.
MICMAC
ETCHEMIN
ABNAKI
PENNACOOK
LAKE CHAMPLAIN
CAPE ANN
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
WABANAKIA
Scale: 0 100 Miles
LEGEND
1 St. Croix River
2 Passamaquoddy Bay
3 Merrimac River
4 Quacke / Yorke / Portland on Casco Bay
5 Androscoggin River
6 Sagadahoc Estuary
7 Kennebec River
8 Saco River
9 Penobscot Bay
10 Penobscot River
11 Mt. Desert Island
12 St. John River
13 Port Royal
14 Richibucto
15 Restigouche

## THE PROBLEM

A specialty of the ethnohistorical anthropologist is to apply general theoretical models devised by modern social-and-cultural anthropology to help interpret the early documentary accounts about past non-literate peoples. Without such models, little if any meaningfulness can be wrung from this potential treasure-lode of ethnological raw-materials. However, the necessary interplay of anthropology and history to attain this end is not always automatically forthcoming. On the one hand, anthropological fieldworkers too often shun their library homework, making the dynamics of time-depth perspective far too shallow in their ensuing analyses of remnant native peoples. On the other hand, historical researchers too often eschew non-literate peoples as outside their interests, or else they analyze them too ethnocentrically. When the latter is coupled with the fact that all too frequently the early documents themselves were poorly recorded ethnocentric misunderstandings of incomplete observations, the results of such attempts are poor indeed.

If the sophistications of native institutions and leaders are ignored or underestimated, the resulting stereotype of aborigines as mindless passive straws in the European whirlwind is far from adequate. Indeed, the cleverness of native leaders in exploiting the presence of Whites to attain desired native ends has had no small causative significance in the dynamics of culture contact, and ethnohistorical anthropology can shine much-needed light on this subject, if anything can.[2] A logical starting point for such analysis, and the sole concern of this paper, is with the status and role of native leaders in a particular sociocultural context.

Our focus is upon sociopolitical leaders called ***sagamores*** among the seventeenth-century East-North-East Algonkians, collectively termed the ***Wabanaki*** or Dawnlanders, of sub-St. Lawrence Canada and northern New England. Heretofore, no such model-using ethnohistorical analysis has been applied to Wabanaki leadership during the Early Historic Period. The few anthropologists dealing with this area and/or period have attended to other matters; the area's historians have tended either to avoid or to inadequately appreciate the complexities involved in the occasionally-quite-contradictory accounts written during the period by European observers of various biases. Further, this default or ethnocentrism on the part of scholars has allowed the unbridled romanticism of popular writers to declare that kings—even emperors—were the rulers of the forests of Wabanakia. It is hoped, therefore, that the present study will start a more rational approach.

## THE WABANAKI

At the beginning of the Early Historic Period (starting ca. 1600), the land of the Wabanaki peoples (the Dawnland territory here called Wabanakia) stretched north-south from Gaspe Peninsula to Cape Ann, and east-west from Cape Breton Island to or toward Lake Champlain, but the earliest meaningful accounts tell only of the immediate coastal areas.[3] Three ethnic and linguistic blocs are distinguishable among the Wabanaki, from east to west: Micmac, Etchemin, and Abnaki-Pennacook.[4] Native political affiliations and military alliances seem to have been somewhat unstable and opportunistic. Dawnlander sagamores clearly appear to have been rival one-ups-men, ever mindful of their own personal interests and reputations.[5]

The Wabanaki peoples of today are known as the Micmac, Maliseet, Passamaquoddy, Penobscot, and Abenaki, and they still occupy token vestiges of their aboriginal homelands.[6] In the seventeenth century, European observers describe what are here called the Micmac as residing from Gaspe Peninsula on the north and Cape Breton Island on the east throughout eastern New Brunswick, Prince Edward Island, and all of Nova Scotia. The early seventeenth-century Etchemin, who apparently were ancestral to the modern Maliseet and Passamaquoddy, occupied the land from the St. John River in New Brunswick to the Kennebec River in Maine. They were divided approximately at Mt. Desert Island (Maine) into Eastern Etchemin allied with the Micmac (Alliance 1B on the chart), and Western Etchemin whose Penobscot River-based leader Bashaba seems to have been at least nominal head sagamore of what may be called the Abenaki Confederacy—a polyglot alliance of bands and peoples of southern Maine (Alliance 1A on the chart). From the Kennebec River in Maine to the Merrimac River in New Hampshire and northeastern Massachusetts dwelt the Abnaki-Pennacook peoples, whose eventual displacement and regrouping gave rise to the St. Francis Abenaki and the Penobscot. No easily distinguishable boundary between Abnaki and Pennacook bands can now be made, so they are grouped together here, for the seventeenth century. Some of these bands, but by no means all, were allied with Western Etchemin in Bashaba's Abenaki Confederacy, until its collapse ca. 1615, and frequently were at war with Micmac and Eastern Etchemin.

## *THE MODEL*

While too much concern with general theoretical expectations can blind an observer to the presence of specific actual exceptions to the "rules," too little theoretical structuring can hinder potential progress in analyzing descriptions of observations. Thus there still is merit to some ethnological rules-of-thumb, even though there are occasional exceptions to them. One such rule is that the higher categories of sociopolitical organization (e.g., Elman R. Service's *chiefdom, state,* and *civilization-empire*) are not theoretically expected to be found in societies with food-collecting economies, but only in those which are food-producing. However, the Pacific Northwest Coast food-collectors actually were above the lower sociopolitical categories (e.g., Service's *band* and *tribe*), and were organized as petty *chiefdoms* in their exceptionally bountiful environment (Sanders & Marino 1970:23).[7] Use of any general theoretical model should be cautious in any specific case study. The model is only the beginning, not the end of understanding, yet it is the analytical vehicle needed to rise above the purely descriptive level toward the level of explanation.

Wabanakia is at the northeastern limits of food-production climate, and only the southwestern Wabanaki appear to have grown maize aboriginally (as a supplement to their adequate-but-not-affluent food-collecting economy). Thus there is no theoretical reason to expect that the Dawnlanders had a higher level of sociopolitical integration than Service's *tribe*. However, this at first seemingly low-level classification should not obscure the fact that by 1600, what we can call *tribal confederations* actually had developed throughout the Northeast Woodlands to an interestingly high degree. The Iroquois League of Five Nations seems to have outstripped all others in both complexity and longevity, yet it was only a variation on the commonly-repeated confederacy theme. While both indirect and direct contact influences of the European fur trade obviously fostered the development of these confederations from 1500 onward, a much earlier (and aboriginal) stimulus—ending ca. 1500— must have been the alliances and extensive trade networks of the prehistoric "Mound-Builder" cultures (Adenan, Hopewellian, and Mississippian).[8] Thus, both theoretically and actually, *tribe-level confederations* can be considered the norm for the seventeenth-century Wabanaki. These seem to have ranged between simple and complex extremes, apparently depending to a great deal upon the personal influence of individual leaders therein.

Lack of observers' documentation before 1600 makes the sixteenth-century Wabanaki sociopolitical scene a mystery, but some English accounts starting ca. 1605 tell of an extraordinary alliance, a polyglot super-confederation that may be termed Bashaba's Abenaki Confederacy. Contemporary French accounts refer to Bashaba merely as a local sagamore of the Penobscot River and Bay area, albeit one of prominence. In this paper, Bashaba will be accepted as actual head sagamore of a super-confederation, but he will be considered as both unique and effemeral, and to represent the zenith of *recorded* Wabanaki leadership. "Head sagamore" herein equates only as official first-among-equals, whatever more prestige it may imply. How much of his influence came from personal charisma and how much was socially structured can only be surmised. After Western Etchemin Bashaba was slain by marauding Micmac, ca. 1615, his super-confederation seems to have collapsed forever. European intrusion—both plague and politics—prevented any one Wabanaki sagamore ever again from amassing as much supposed individual leadership priority, although there are hints that later Penobscot Bay area sagamores were deferred to by other Wabanaki, perhaps as a structural carryover from Bashaba's days and earlier.[9]

The clearer picture of Wabanaki sagamores' status and role that comes to us from later seventeenth-century documents seems at first consideration to be almost antithetical to the apparently high position described above for Bashaba. Yet the area's history for that century is full of the names and acts of Wabanaki apparent-strong-men; clever leaders need only opportunities, not mandates, to gain control over others. Influence, not authority, is the key concept necessary to understand both Bashaba's and later sagamores' leadership activities, and influence can be subtle, multi-dimensional, self-reinforcing, and all-pervasive, among other things. One gets the impression from the documents that Wabanaki sagamores were just as aware of all this as we should be, and well knew how to profit by it.

Morton H. Fried's (1960) seminal article "On the Evolution of Social Stratification and the State" presents a taxonomy of four stages and types of social complexity: *egalitarian, rank, stratified,* and *state* societies. These roughly equate with Service's *band, tribe, chiefdom,* and *state* (here including *civilization-empire*), but for our purposes Fried much more meaningfully depicts the respective leadership statuses and roles. Certainly no better leadership model can be found to interpret the accounts of Wabanaki sagamores of the 1600s than that which Fried (1960:718-719) gives for the *rank society*. Here are excerpts.

> The key status is that of the central collector of allotments who also tends to the redistribution of these supplies either in the form of feasts or as emergency seed and provender in time of need....
>
> In the typical ranked [sic] society there is neither exploitative economic power nor genuine political power.... This is not surprising, for the system in typical rank societies is actually based upon a physical expension of the kin group and the continuation of previously known kinship rights and obligations. The kingpin of a redistributive network in an advanced hunting and gathering society or a simple agricultural one is as much the victim of his [economic] role as its manipulator.... This...helps to explain that "anomaly" that has so frequently puzzled students of societies of this grade: why are their "chiefs" so often poor, perhaps poorer than any of their neighbors?...
>
> It is a further characteristic of the persons filling these high status positions in typical rank societies that they must carry out their functions in the absence of political authority. Two kinds of authority they have: familial, in the extended sense, and sacred, as the redistributive feasts commonly are associated with the ritual life of the community. They do not, however, have access to the privileged use of force, and they can use only diffuse and supernatural sanctions to achieve their ends. Indeed, the two major methods by which they operate are by setting personal examples, as of industriousness, and by utilizing the principles of reciprocity to bolster the emergent redistributive economy.
>
> Despite strong egalitarien features in its economic and political sectors, the developing rank society has strong status differentials which are marked by sumptuary specialization and ceremonial function. While it is a fact that the literature abounds in references to "chiefs" who can issue no positive commands and "ruling classes" whose members are among the paupers of the realm, it must be stated in fairness that the central redistributive statuses *are* associated with fuss, feathers, and other trappings of office.

Fried characterizes the rank society as having fewer positions of valued status than individuals capable of handling them. So, succession to high status may be dependent upon birth order—either primogeniture or ultimogeniture—on the level of the family, extended family, or lineage. Sometimes only the first son of a first son enjoys the right of succession. If election determines the accession to high status, the candidates may come only from certain lineages which already represent selection by birth order (Fried 1960:717).

> The effects of rules of selection based on birth can be set aside by conscious action. Incompetence can be the basis for a decision to by-pass the customary heir, though it would seem more usual for the nominal office to remain vested in the proper heir while a more energetic person performed the functions of the status. A strategic murder could also accomplish the temporary voiding of the rule, but such a solution is much too dangerous and extreme to be practical on the level which we are considering. It is only in rather advanced cultures that the rewards associated with such statuses are sufficient to motivate patricide and fratricide.[10]

Given the status-role structure in Fried's model, it remains to find a model of motive to make it all work, and a very succinct one is available. Study of seventeenth-century Wabanaki sagamores and their decisions made to try to hold native culture together while optimumly exploiting European contacts for both the collective good and selfish individual advantage (Morrison 1974) shows that they well exemplify the "*politically rational man*—one who would rather win than lose," a concept carefully scrutinized by Ronald Cohen (1973:873 and 1970:496). At first glance, this may seem too tritely Western, but it applies equally well in Wabanakia, if not even better. Making "winning" decisions was the sure way for a sagamore to enhance his prestige and influence in his own community and in nearby bands, although, antithetically, it could also increase the jealousies and rivalries of both factionalists at home and neighboring sagamores.

Over and above the acts of perfidy between races—the stuff of classic Red-White "history"—the early accounts also tell of rival sagamores' treachery toward each other.[11] Validation—even approval and admiration—for a sagamore's sophisticated duplicity in his dealings with potential enemies (both individual and collective) can be explained in terms of Bronislaw Malinowski's (1926 *in* 1948:144ff) concept of the "myth as charter" for the belief and act. Gluskap, the Wabanaki culture-hero, also combined trickster attributes, and thus successfully double-dealt with his opponents in the Dawnlanders' folklore. With such behavior practiced by one's supernatural superior, and not forbidden to mortals to imitate, and presented with positive reinforcement in oral tradition, it is no wonder that it became a potential *modus operandi* in the realpolitik of the Wabanaki sagamore. And while, in practice, a person or group defined as enemy might be treated both honorably and respectfully by the Wabanaki (or innumerable other peoples known to anthropology), in theory, honor and respect certainly were not automatically due to an enemy. By simple extension, such potential enemies as foreigners and political opponents could easily fall outside the pale of faithfulness, too.

In combination, then, Fried's structure model of leadership in the "rank society," Cohen's motive model of the "politically rational man," and Malinowski's validation model of the "myth as charter" for perfidiously dealing with "enemies," collectively give us a theoretically logical starting point for meaningfully analyzing the earliest written accounts of the status and role of Wabanaki sagamores.

## *DATA AND INTERPRETATION*

### *Wabanaki Sagamores and Political Structure*

William Wood, an Englishman most familiar with the Massachusetts Bay area Amerinds, but also rather cognizant of the Pennacook, wrote the following caricature of a sagamore (1639:57; emphases in original):

> ...a Sagamore with a Humberd in his eare for a pendant, a blacke hawke on his *occiput* for his plume, Mowhackes, for his gold chaine, good store of Wampompeague begirting his loynes, his bow in his hand, his quiver at his back, with six naked *Indian* spatterlashes at his heeles for his guard, thinks himselfe little inferiour to the great *Cham*, he will not sticke to say he is all one with King *Charles*. He thinks he can blow down Castles with his breath, and conquer kingdomes with his conceit.

Wood's vignette also caricatures the English, if only indirectly, because seventeenth-century Europeans all too frequently used only their own ethnocentric political, legal, and religious concepts in both their expectations of, and dealings with, Amerinds. Yet, there were a very few observers who approximated a relativistic approach, in some areas of affairs at least. Jesuit missionary Pierre Biard was one of these few, and the earliest in Wabanakia. He dwelt in the country of the Micmac and the Etchemin during 1611-1613 and also briefly visited the Abnaki. Biard seems to have been rather adept at grasping the native political organization and the sagamore's place within it. Some of his insightful statements follow (1616 *in* JR 3:87-91,99-101).

> . . .[The sagamore is] the eldest son of some powerful family, and consequently also its chief and leader. All the young people of the family are at his table and in his retinue; it is also his duty to provide dogs for the chase, canoes for transportation, provisions and reserves for bad weather and expeditions. The young people flatter him, hunt, and serve their apprenticeship under him, not being allowed to have anything before they are married, for then only can they . . . do for themselves. Nevertheless they continue to live under the authority of the Sagamore, and very often in his company; as also do several others who have no relations, or those who of their own free will place themselves under his protection and guidance, being themselves weak and without a following. Now all that the young men capture belongs to the Sagamore; but the married ones give him only a part, and if these leave him, as they often do for the sake of the chase and supplies, returning afterwards, they pay their dues and homage in skins and like gifts. From this cause there are some quarrels . . . [as when] . . . some one begins to assert himself and to act the Sagamore, when he does not render the tribute, when his people leave him or when others get them away from him. . . .
>
> It is principally in Summer that they pay visits and hold their State Councils; I mean that several Sagamores come together and consult among themselves about peace and war, treaties of friendship and treaties for the common good. It is only these Sagamores who have a voice in the discussion and who make the speeches, unless there be some old and renowned Autmoins [shamans], who are like their Priests, for they respect them very much and give them a hearing the same as to the Sagamores. It happens sometimes that the same person is both *Autmoin* and *Sagamore*, and then he is greatly dreaded. . . . Now in these assemblies, if there is some news of importance, as that their neighbors wish to make war upon them, or

> that they have killed some one, or that they must renew the alliance, etc., then messengers fly from all parts to make up the more general assembly, that they may avail themselves of all the confederates, which they call *Ricamanen*, who are generally those of the same language. Nevertheless the confederation often extends farther than the language does, and war sometimes arises against those who have the same language. In these assemblies so general, they resolve upon peace, truce, war, or nothing at all, as often happens in the councils where there are several chiefs, without order and subordination, whence they frequently depart more confused and disunited than when they came. . . .
>
> According to the custom of the country, they can have several wives, but the greater number of them that I have seen have only one; some of the Sagamores pretend that they cannot do without this plurality, not because of lust (for this nation is not very unchaste) but for two other reasons. One is, in order to retain their authority and power by having a number of children; for in that lies the strength of the house, in the great number of allies and connections; the second reason is their entertainment and service, which is great and laborious, since they have large families and a great number of followers, and therefore require a number of servants and housewives. . . .

Elsewhere, Biard (1612b *in* JR 2:73; emphases added) raises the issue of voluntary obedience to sagamores:

> . . . among them, each man is his own master and his own protector. They have Sagamores, that is, leaders in war; but *their authority is most precarious, if, indeed, that may be called authority to which obedience is in no wise obligatory. The Indians follow them through the persuasion of example or of custom, or of ties of kindred and alliance; sometimes even through a certain authority of power, no doubt.*

Recollet missionary Chrestien LeClercq not only confirms Biard's earlier accounts of native politics and the voluntary obedience of the sagamore's constituency, but explicitly states a sociopolitical change away from the formalities of both councils and authoritative sagamores toward a more free-wheeling form of political organization, among the northernmost Micmac, in the late seventeenth century. LeClercq (1691/1910:234-235; emphases added) attributes this to a general decadence caused by departure from ancestral traditions; i.e., cultural breakdown due to contact stimuli.

> One sees no more among these people those large assemblies in the form of councils, nor that supreme authority of the heads of families, elders, and chiefs, who regulated civil and criminal affairs, and in the last resort decided upon war and upon peace, giving such orders as they thought absolutely essential, and enforcing the observance thereof with much submission and fidelity. There are now only two or three Indians who, in their own districts still preserve, though feebly, a sort of power and authority, if one can say that such is found among these peoples. The most prominent chief is followed by several young warriors and by several hunters, who act always as his escort, and who fall in under arms when this ruler wishes particular distinction upon some special occasion. But, in fact, *all his power and authority are based only upon the good will of those of his nation, who execute his orders just in so far as it pleases them.*

Wabanaki society had three general social strata: l) the *elite* (sagamores, shamans, highest elders, and their closest kinfolk), 2) the *commoners* (the entire remainder of the tribe), and 3) the *captives* (prisoners of war from hostile tribes, sometimes including other Wabanaki tribes). While some upward mobility did occur, even from the captive stratum, elite lineages were the usual source of future leaders; inheritance of supernatural powers usually ran in certain elite families, too. French entrepreneur Nicolas Denys wrote of Micmac interest in elite genealogy: "On these matters they are very inquisitive, especially those descended from the ancient chiefs; this they sometimes claim for more than twenty generations, something which makes them more honoured by all the others" (Denys 1672/1908:410).

Perhaps the most succinct comment on the snobbishness of the elite stratum is that of Englishman Christopher Levett. He lived at what is now Portland, Maine in 1623-24, at the invitation of the local Abnaki sagamore, who visited with Levett frequently, and usually in the company of other sagamores. Levett states, "The *Sagamores* will scarce speake to an ordinary man, but will point to their men, and say *Sanops*, must speake to *Sanops* and *Sagamors* to *Sagamors*" (Levett 1628 *in* Baxter 1893:117).[12]

Wabanaki women held a lower overall status than men, and their upward mobility was directly and strongly effected by the statuses of their fathers and husbands. The marriage of an elite daughter was carefully planned, because (at least in some cases) she could inherit important prerogatives. Levett (1628 *in* Baxter 1893:104-105) tells of such a situation, wherein a woman inherited a sagamoreship from her father, but seemingly shared the office (or the title) with her husband, who also was a sagamore elsewhere, in his own right.

> . . . *Cogawesco* [the man Levett calls "king"], the *Sagamore* of *Casco* and *Quacke*, told me if that I would sit downe at either of those two places, I should be very welcome, and that he and his wife would goe along with me in my boate to see them. . . . [Levett decided, sight unseen, to settle at Quacke, which he planned to call Yorke instead.]
>
> The next day the winde came faire, and I sayled to *Quacke* or *Yorke*, with the King, Queene, and Prince, bowe and arrowes, dogge and kettell in my boate, his noble attendance rowing by vs in their Cannow.
>
> When we came to *Yorke* the Masters of the Shippes came to bid me welcome, and asked what Sauages those were, I told them, and I thanked them, they vsed them kindly, & gaue them meate, drinke and tobacco. The woman or reputed Queene, asked me if those men were my friends, I told her they were; then she dranke to them, and told them, they were welcome to her Countrey, and so should all my friends be at any time, she dranke also to her husband, and bid him welcome to her Countrey too, for you must vnderstand that her father was the *Sagamore* of this place, and left it to her at his death hauing no more Children.

### *Micmac Sagamores*

No clearer indication of the range of variation in leadership types among seventeenth-century Wabanaki sagamores could be asked for than the two contrasting accounts which follow. Both of these Micmac sagamores were roughly contemporaries, in the latter half of the 1600s, and must represent extreme polar opposite types. The benevolent and unnamed Restigouche Micmac "chief," described by Recollet LeClercq (1691/1910: 235-236; emphases added), is here designated as Type A.

> We had among us, at the River of Saint Joseph [Restigouche], one of these old chiefs whom our Gaspesians considered as their head and their ruler, much more because of his family, which was very numerous, than because of his sovereign power, of which they have shaken off the yoke, and which they are not willing any longer to recognise.
>
> The occupation of this chief was to assign the places for hunting, and to take the furs of the Indians, giving them in return whatever they needed. This man made it a point of honour to be always the worst dressed of his people, and to take care that they all were better clothed than he. He held it as a maxim, as he told me one day, that a ruler, and a great heart like his, ought to take more care for others than for himself, because, good hunter as he was, he always obtained easily everything which he needed for his own use, and that as for the rest, if he did not himself live well, he should find his desire in the affection and the hearts of his subjects. It was as if he wished to say that his treasures and riches were in the hearts and in the affection of his people.
>
> It happened that a stranger wished to dispute his right of command, or at least to share with this ruler that government, with its imaginary grandeur, for which he had as much regard as if it were the greatest empire of the world. This competitor arrived well provided with axes, with guns, with blankets, with beavers, and with everything which could give him some prominence and some entrance into the sovereignty which he claimed was properly due him by right of hereditary succession, because his father had been formerly head and chief of the Gaspesian nation. "Very well," said our Indian, "show that thy heart is a true chief's heart and worthy of absolute empire over the people whom I rule. There," continued he, "are some poor Indians who are wholly naked; give them thy robes of otter and beaver. Thou seest, again, that I am the worst dressed of all, and it is through this that I wish to appear chief—through despoiling and depriving myself of everything in order to aid my Indians. Therefore, when following my example, thou shouldst be as poor as I. Let us go a-hunting when the time is right, and the one of us who kills the most moose and beavers shall be

> the legitimate king of all the Gaspesians." The stranger accepted this challenge with spirit. In imitation of our chief, he gave away everything he had, and kept back nothing except the bare necessaries. He went hunting, but he was so unfortunate as to do it very badly, and consequently he was obliged to abandon the enterprise which he had formed of commanding our Gaspesians, who did not wish to recognise any other head than their *old and brave chief whom they obeyed with pleasure.*

The "conceited and vicious" Richibucto Micmac "chief" Denis, portrayed by French entrepreneur Nicholas Denys (1672/1908:195-196; emphases added), is here called Type Z.

> The Chief at Rechibouctou, named Denis, is a conceited and vicious Indian. All the others of the Great Bay fear him. He has upon the border of the basin of this river a rather large fort of stakes, with two kinds of bastions, inside is his wigwam, and the other Indians are encamped around him. He has had a great piece of wood placed upright to the top of a tree, with large pegs which pass through it in the manner of an estrapade and serve as steps for ascending to the top. There from time to time he sends an Indian to see if he can perceive anything along the coasts. From this place one can see far out to sea. If any vessels or canoes are seen, he has his entire force brought under arms with their bows and arrows and their muskets, places a sentinel on the approach to ask what persons they are, and then according to his whim he makes them wait, or has them come immediately. Before entering it is required that they make a discharge of their guns, as a salute, and sometimes two. Then the leader enters, and his suite after him. He never goes out from his wigwam to receive those who come to visit him. He is always there planted upon his haunches like an ape, his pipe in his mouth if he has any tobacco. He never speaks first. He expects that he shall be paid a compliment, and sometime later he replies with the gravity of a magistrate.
> If he goes to the wigwam of some Indian, on arriving he has a musket discharged to inform the other Indians, who come out from their wigwams, and go to meet him with their muskets. Then he lands from his boat and sets foot upon shore, and all the Indians who are there discharge their muskets. Then they accompany him to the wigwams, [and] when he goes inside they again fire each one a shot from his musket. Such is the manner in which he makes them receive him, *more through fear than through friendship. They all wish for his death; he is not liked by a single one. If they are delinquent in their duty, he beats them, but not when they are together, for in this case he could not do it with impunity.* But when he catches them alone he makes them remember their duty. If the Indians make a debauch, he is never of their number, [but] he hides himself, for in drunkenness they are as great chiefs as he, and if he were to say to them something which made them angry, they would murder him. At such times he is wise, and never speaks of his greatness.

It is not by choice but by necessity that the Micmac receive more attention than the other Wabanaki peoples in this analysis. The Micmac are the only Wabanaki people about whom enough seventeenth-century data are known to enable detailed ethnographic descriptions of any sort. Some of the early Frenchmen among the Micmac were, indeed, protoethnographers, while early English writers, elsewhere in Wabanakia, were not. The probable reasons for this difference are outside the scope of the present work, but the existence of the difference and its effect of forcing a Micmac orientation must be seen as important facts of life. Bernard G. Hoffman's (1955) lengthy *Historical Ethnography of the Micmac* thus is not only the sole modern general study of a Wabanaki people based on ethnohistorical documents, but also is the only possible general ethnography of the Wabanaki in the Early Historic Period. Detailed succinct ethnological summaries of Micmac sociopolitical affairs abound in Hoffman's work, and because of their direct relevance to the present study, Hoffman will be quoted or paraphrased at some length. However, certain necessary pruning and grafting has been done to enable the contention here that these findings quite probably can rightly be extended to apply to *all* of the seventeenth-century Wabanaki peoples.[13]

Hoffman (1955:589-590,705-706) claims bilateral descent for the early Micmac. Early accounts telling of the freedom to change from one sagamore's following to another's imply flexibility of both descent rules and residence rules. According to Hoffman (1955:590):

> It is thus quite likely that the basic unit in ancient times was a residential kin group consisting of a chief, his family, various of his married sons and their families, some of his married daughters and their families (whose husbands found it advantageous to reside in his village), and other relatives in the matrilineal and patrilineal line who desired his protection and benefits. Such a unit has been termed a *bilocal extended family* by Murdock (1949, pp. 34-35).

Perhaps the majority of Micmac sagamores were born into chiefly lineages. However, the ascribed status deriving from elite and adequate kinship affiliations provided only an opportunity. A would-be sagamore also needed the achieved status of earned public recognition for manifesting superior personal ability: in leading men by his example; in inspiring confidence in his wisdom and physical prowess; in proving his concern for and generosity toward others; in attaining success in hunting, warfare, and supernatural affairs. The greatest Micmac sagamores were themselves also shamans and *ginaps* (war leaders); less eminent sagamores merely controlled the services of these specialists (Hoffman 1955:514-517 ).

> The territory or domain of local chiefs seems to have been coextensive with the area commonly occupied by the inhabitants of a single summer village. Within this village the local chief was the recognized head of the "council of elders," which consisted of the heads or representatives of the families represented in the settlement. We conclude, from the comment made by Biard, that decisions of the council depended upon a unanimous vote on the part of the members, and that only such decisions were regarded as giving the chiefs authority to act upon a certain matter. We may therefore ***characterize Micmac political organization as being dependent upon the voluntary association of the adult male members of the community—women, children, and young men who had not yet killed their first moose having no part in the system. The latter were apparently required to obey without question the decisions of the councils and of the chiefs.*** [Hoffman 1955:516; emphases added]

Elsewhere, Hoffman (1956:190) adds that Micmac "local chiefs tended to align their people into regional groupings or alliances; occasions are known when such groupings were dominated by chiefs with dominant personalities, powerful families, or unusual supernatural powers." This organizational principal can be seen as the nub of the various "confederations" and "confederacies" mentioned earlier. It, like the other Hoffman passages above, seems to be applicable not just to the Micmac, but to all of the Wabanaki peoples of the seventeenth century.

### *Voluntary Obedience*

The only perplexing issue about sagamoreship raised here thus far is the point about voluntary obedience: "for they obey, as we have said, only their head men and their chiefs in so far as it pleases them" (LeClercq 1691/1910:242). French Roman Catholic missionaries Biard (1612b *in* JR 2:73) and LeClercq (1691/1910: 234-239) both agree that obedience of the wishes of one's sagamore usually was not required of an adult male Micmac, in their respectively early and late seventeenth-century eyewitness accounts of the Micmac. And yet French entrepreneur Nicolas Denys (1672/1908:195-196) tells of Micmac Chief Denis of Richibucto, who ruled with an iron hand—but only through the apathetic tolerance of his constitutents, apparently. Even earlier French visitors among the Micmac, royal explorer Samuel de Champlain and adventuring lawyer Marc Lescarbot, add support to the voluntary obedience thesis: "They have chiefs whom they obey in regard to matters of warfare but not in anything else." (Champlain 1613/1922:413); "This Sagamos has not absolute authority among them, but . . . guide[s] the people rather by example than by commandment" (Lescarbot 1618a/1907-14 3:265).

Were it not for Fried's model of leaders in the rank society—"they must carry out their functions in the absence of political authority"—voluntary obedience to Wabanaki sagamores would be hard to interpret. Especially is this so in the light of an early English account based upon contact with Western Etchemin and Abnaki. The chronicler of Captain George Waymouth's 1605 reconnaissance voyage to the Maine coast area between the Penobscot and Kennebec Rivers was one James Rosier. Of the Amerinds, he states: "They shew great reverence to their King, and are in great subjection to their Governours: and they will shew a great respect to any we tell them are our Commanders" (Rosier 1605a *in* Burrage 1906:392). This statement, right or wrong, is suspiciously extreme. On the return voyage to England, Rosier had on board with him the five captive native "informants" whom Waymouth kidnapped for purposes of interrogation and future guiding of colonial parties.[14] Rosier undoubtedly worked closely with the captives, both then and later, to gain answers to many questions. Rosier's job was to write propaganda to advertise English colonial ventures on the Maine coast. Thus it seems likely that he learned, directly or indirectly, about voluntary obedience to sagamores and that it seemed to him to be potential anarchy. So, feeling that this might discourage prospective colonists, Rosier prevaricated, making the statement in question unique among all the others, because it says the opposite of what they say.

Analysis of another text seems indirectly to explain both the working of voluntary obedience and Europeans' amazement thereat. English fur-buyer Christopher Levett, who resided among the Abnaki at what is now Portland (Maine) in 1623-24, wrote the following (1628 *in* Baxter 1893:117):

> Their *Sagamors* are no Kings, as I verilie beleeue, for I can see no Government or Law amongst them but Club Law: and they call all Masters of Shippes *Sagamore*, or any other man, that they see have a commaund of men.

If we really ponder the old adage that "with authority goes responsibility," and, in the quotation above, read *have a responsibility for men* in place of "have a commaund of men," we can get at the very nub of powerless leadership, and how it can work. Undoubtedly Europeans, like Levett, were conditioned to look at the authority side of the coin, and the Wabanaki (theoretically, at least) were conditioned to look at the responsibility side.

A relatively fluid and voluntary association of men delegated to a sagamore whom they respected the responsibility for their welfare. As long as the responsibility side of the coin stayed uppermost, all was well. If the skillful leader spoke only "responsibility" (regardless of what he thought to himself), and his followers heard only "responsibility," then the leader kept his followers, and the followers voluntarily did the bidding of their leader. The political "rules of the game" (Bailey 1969) were being played by both parties. But if the authority side of the coin showed its face, the followers felt cheated, and the "game" could easily fall apart between leader and led.

The skill required for the model sagamore's responsibility-leadership (unsupported by sovereign authority) obviously was immense, and stress-wise probably was very costly. Supportive props were sought by many incumbents, e.g., supernatural power: "It happens sometimes that the same person is both *Autmoin* [shaman] and *Sagamore*, and then he is greatly dreaded" (Biard 1616 *in* JR 3:91). Richibucto chief Denis' prop was bullying, and enough of his followers apparently were so unusually apathetic that he could get away with it. Yet to Europeans, Chief Denis (our Type Z leader) was much more understandable than LeClercq's unnamed benevolent sagamore of Restigouche (our Type A leader), because of the prevailing European leadership patterns. The need to emphasize chiefly eliteness was differentially felt, and some sagamores resorted to putting *on airs*, such as not desiring to speak to commoners, or by wearing fancier costumes than commoners. There seems to have been much leeway for the personality of the individual sagamore to express itself. Undoubtedly, too, certain families and certain localities and regions developed idiosyncratic traditions, either in the expression of the status or in the fulfillment of the role. And, certainly, over both time and space, some differences gradually would appear.

Paying deference to the sagamore (or to his office) was standard procedure, at least by his retinue of young men and by the women. For example, at feasts, sagamores were specially seated and served. Great pride and prestige were derived from genealogical connections with the great sagamores and shamans of the past. Recitals of such kinship ties were a regular part of feasting activities, and probably of folklore-telling as well. Thus past and present were kept continuous, and the authority of tradition reinforced the tradition of "authority" and "responsibility," to the great supportive benefit of the current leaders. After firearms became commonplace, gunshot salutes were usually offered wherever a sagamore visited. The European explorers and traders inevitably enhanced the chiefly image and deference-doing by giving presents of fancier than average sort (e.g., a dress coat, or a formal hat) to the sagamores alone.

Sagamores from ancient times had at least councils of elders, if not deputy leaders, and probably both. Senior males in a democratic milieu easily become a public forum and a backdrop for any other, delegated, form of leadership. The role of the council will be mentioned later, specifically in connection with organization for hunting and warfare.

The sagamore's retinue of young men, described by Biard (1616 *in* JR 3:87-89) and LeClercq (1691/1910: 234-235) and quoted earlier, acted as their leader's military escort. In return for their services, the sagamore supplied and equipped his retinue and—according to Hoffman (1955:273-274)—"provided for the families of their fathers if necessary, and, more importantly, freed them for the opportunity of gaining honors in warfare." Wood spoke of a six-man guard for his caricature sagamore. Biard's "great sagamore Meteourmite" of the Sheepscot River (near the Sagadahoc Estuary) greeted his guests with a guard of "about forty" (1612a *in* JR 2:41):

> When we arrived, Monsieur de Biancourt armed himself, and thus arrayed proceeded to pay a visit to Meteourmite. He found him in the royal apparel of savage majesty, alone in a wigwam that was well matted above and below, and about forty powerful young men stationed around it like a body-guard, each one with his shield, his bow and arrows upon the ground in front of him. These people are by no means simpletons, and you may believe us when we say so.

Unfortunately, Biard does not describe "the royal apparel of savage majesty" worn by Meteourmite, but Wood's bedecked caricature sagamore is contrasted with his "naked" companions. So, too, is the presumed "Captaine" in this 1593 description of the Cape Breton Island Micmac encountered by the English vessel *Marigold*: "One of the Savages, which seemed to bee their Captaine, ware a long mantle of beastes skinnes on one of his shoulders. The rest were all naked except their privities, which were covered with a skinne tyed behinde" (Fisher 1593 *in* Hakluyt 1903-05 8:159).

### *The Sagamore's Leadership Roles*

As sagamore *per se*, a Wabanaki leader's peacetime political duties were both commonplace and ceremonial, as has been stated or implied earlier. Oratory and statesmanship were necessary talents of a sagamore, both at home and when away visiting. Maintaining "a grave and dignified demeanor" was ever the expected chiefly conduct.

Organization for subsistence activities appears to have been a joint responsibility of sagamore and council of elders. Decisions regarding seasonal movements of the group and assignments of hunting territory were made carefully. LeClercq (1691/1910:237) clearly states that the Gaspesian Micmac did not allow voluntary obedience in these economic affairs, at least in the latter half of the 1600s:

> It is the right of the head of the nation, according to the customs of the country, which serve as laws and regulations to the Gaspesians, to distribute the places of hunting to each individual. It is not permitted to any Indian to overstep the bounds and limits of the region which shall have been assigned him in the assemblies of the elders. These are held in autumn and spring expressly to make this assignment.

In return for equipping hunting parties, even LeClercq's benevolent Restigouche chief (our Type A leader description) took their furs. This may have been totally a response to European fur trade contact stimuli, but it clearly places the sagamore in the category of redistribution agent. Biard (1616 *in* JR 3:87-89), early in the century, spoke of payment to the sagamore of "dues and homage in skins." The implication by both writers is that this was a system of mandatory, not voluntary, tribute and redistribution. Yet LeClercq's benevolent chief did not himself cease to "produce" food. Instead, he was a superlative hunter, and this probably would have been attributed to his having strong shamanistic powers over the game—powers which his rival lacked.

In military affairs, the sagamore had a complex homefront role at least, separate from any additional battlefront role that he might or might not have. Warfare was not entered into lightly, although after it was initiated, raiding parties could be launched impetuously. The role of the council of elders in deciding upon whether or not to initiate a war seems to have been more important than the role of the sagamore. Thereafter, too, the Micmac council continued to advise and consent, according to LeClercq (1691/1910:269,237).

> War, however, is never declared except by advise of the old men, who alone decide, in the last resort, the affairs of the country. They prescribe the order which must be followed in the execution of their military undertakings; they fix the day of departure; and they assemble the young warriors to the war feast.
>
> The young people must strictly obey the orders of the chiefs. When it is a question of going to war, they must allow themselves to be led, and must attack and fight the nation which they wish to destroy, in the manner which has been planned by the head of their council of war.

Upon approval (if such was forthcoming) from the council, of a declaration of war, the sagamore became an active party indeed. Lawyer Lescarbot (1618a/1907-14 3:264) describes this early stage activity as he saw it in 1607.

> When therefore they wish to make war, the Sagamos most in credit among them sends the news of the cause and the rendezvous, and the time of the muster. On their arrival he makes them long orations on the subject which has come up, and to encourage them. At each proposal he asks their advice, and if they consent they all make an exclamation, saying *Hau*, in a long-drawn-out voice; if not, some Sagamos will begin to speak, and give his opinion, and both are heard with attention. Their wars are carried on solely by surprises, in the dead of night, or if by moonlight, by ambushes, or subtility. . . .

Another visitor to the land of the Micmac, surgeon Sieur de Diereville (1708/1933:156-157), updates Lescarbot's account to ca. 1700:

> The Sagamores who take offense
> At some outrage, or hostile acts
> Committed by New England on their Land,
> Summon their Followers for war.
> The better to arouse them, an Harangue
> Is given, and Indian Rhetoric
> Makes use of all its finest turns;
> It's bold, and it's pathetic too. . . .
>
> These Sagamores inspire this noble resolve in those they govern; for each one has his own district, & his People raise the Hatchet, and ask the others if they do not wish, like themselves, to take it in hand.'

And, writing in 1758, about events supposedly "not long ago," but maybe in 1646, secular priest Antoine Maillard describes the role of the oldest sagamore present at the Micmac ceremony as leading a ritual sacrifice to the sun at the War Feast, after a lengthy speech of exhortation (Maillard 1758:19-30).

Returning with his victorious forces, from the war, the Micmac sagamore led the ceremonies, according to LeClercq: "Then they make feasts for the dead, at which the chief sets forth in his speech the fine actions of those who have distinguished themselves and who have been killed in the combat" (LeClercq 1691/1910: 270-271). In New England, Wood (1639:72) relates (and other sources agree) that the victorious chief brought home grisly souvenirs from the war.

> These use no other weapons in warre than bowes, and arrowes, saving that their Captaines have long speares, on which if they returne conquerours they carrie the heads of their chiefe enemies that they slay in the wars: it being the custome to cut off their heads, hands, and feete, to beare home to their wives and children, as true tokens of their renowned victorie.

### *The Sagamore as Ginap (War–Leader)*

In the actual fighting itself, the sagamore may or may not have played a major role, depending upon whether or not he was a *ginap* as well as a sagamore. A *ginap* was a war-leader endowed with supernatural military powers. A sagamore usually was a *ginap*, but a *ginap* was not necessarily either a sagamore or even a shaman. The distinction does not seem to be that between "peace chiefs" and "war chiefs," as known in some Amerind cultures.

With consideration of the *ginap*, we really move out of ethnohistory and into the realm of folk-history, as these terms are distinguished by Charles Hudson (1966). Hudson considers ethnohistory to be the outside, more nearly objectively analytic, "etic" approach to a society's past. He considers folk-history to be the inside, more usually subjectively traditional, "emic" approach. Both approaches need to be considered in any well-rounded study. Especially is this so in the case of the *ginap*, which is such an extremely important concept in both Micmac and Maliseet folklore and oral tradition. The sociocultural attitudes pertaining to the *ginap* in the past must have influenced the conduct of military affairs at the most realistic and pragmatic level.[15]

Malinowski's validation model of the "myth as charter" for the belief is the simplest explanation for this phenomenon of a self-fulfilling prophecy. Whether or not a *ginap* (or a non-military shaman, too) "really" had great supernatural power mattered not one whit. Provided that enough of his opponents and followers attributed him as having it and responded accordingly, they would all respect his legendary shadow whatever his actual substance might be. Like Napoleon, a *ginap* (or a shaman, too) could become legendary in his own time, and conquer over great odds solely because of his reputation's effect.

William H. Mechling's (1958-59:141-142) description of the *ginap* is perhaps the best available.

> They were really not chiefs at all, for they were not political officials. A *ginap* was a man who had, through previous bravery and skill in war, made a reputation for himself, and who, therefore, was able to obtain a following in war time. . . . Even though a *ginap* may not be a *medeulin* [shaman], he is supposed nevertheless to have supernatural power, but only along particular lines. For example, . . . [he might] perform feats of strength which would be impossible to ordinary individuals. How much supernatural power he is supposed to have is well shown by the fact that a war party, no matter how numerous, is entirely helpless before a much less numerous enemy if it loses its *ginap* or *ginaps*. . . . A *ginap* was neither elected nor did he inherit the office from his father, but obtained it entirely through his own abilities in warfare. There was no fixed number at any village or for the tribe as a whole, nor had they any set political powers. The chief was usually, but not always, a *ginap*.

### *The Sagamore as Shaman*

> . . . Every *Sagamore* hath one of them [shamans] belongs to his company, and they are altogether directed by them. [Levett 1628 *in* Baxter 1893:116]

> . . . Every Sagamos has his *Aoutmoin* [shaman], or is one himself. . . . [Lescarbot 1618a/1907-14:3:111]

> It happens sometimes that the same person is both *Autmoin* and *Sagamore*, and then he is greatly dreaded. [Biard 1616 *in* JR 3:91]

> It seems not to have been observed that among our northern tribes the greater chiefs had more repute as magicians than as warriors. Ordinary males could fight; but only the man with magic could contend with the unseen powers and work out destiny. [Eckstorm 1945:99]

From the above passages, the important link between Wabanaki sagamores and shamans can be seen clearly. What is of concern here is the additional status and role of shaman held by some sagamores.

In the animistic religion of the Wabanaki, not only was interaction with everyday spirits necessary, but also with a host of supernatural beings. As a possessor and manipulator of supernatural power, the Micmac shaman is called *Buowin* (with various spellings), and was known to the French as an *Aoutmoin* (with various spellings). The equivalent term for a shaman among the Maliseet, Passamaquoddy, and Penobscot was (and is) *Medeulin* (with various spellings). And, the early English accounts of Abnaki and Pennacook shamans refer to them as *Pow-wows* (with various spellings). Europeans also frequently called shamans "wizards," "witches," "sorcerers," "necromancers," "conjurors," "jugglers," and various other things—usually insisting that they were in league with Satan. Modern Americans frequently call them "medicine-men."

As with the *ginap*, the office of shaman could be entirely separate from that of sagamore. Yet probably no better supportive prop existed for a sagamore than to augment his secular powers with spiritual powers. A successful shaman was publicly reputed to be a curer, diviner, game-caller, and spirit-communicator, and was in fact a people-manipulator *par excellence*. While a sagamore usually could not force his will upon his *sanops*, a shaman frequently could frighten others into doing his bidding (if all else failed). All indications are that the European fur trade and other contacts increasingly changed group-oriented traditional norms among the Wabanaki toward more individualistic and selfish lifeways. If shamans *per se* sought tangible goods as the main end of their lucrative practice, sagamore-shamans sought increasing power over their constituents. Perhaps this attempt was not always consciously willful, but the potential results were too easily adaptive to go totally unheeded. There appears to be nothing in the ethnohistorical record to indicate that Wabanaki sagamores ever were or could have been truly selfless and lacking in ego-gratifying ambitions—even the benevolent Type A leader.

Thus, supernatural enhancement of political power was a respectable and institutionalized means usable by any sagamore to attain career advancement. More directly, however, the sagamore-shaman profitted from the double visibility effect of leading public religious ceremonies as well as political ones. With such concentration of leadership in a single able individual, it is an easy next step to his becoming the unchallenged guardian of the public welfare, whose very advice becomes the moral code of his constituency.

Penobscot oral tradition indicates a truly ancient connection between religious and political leadership, with the former siring the latter. In his book of folk-history, *The Life and Traditions of the Red Man,* Penobscot elder Joseph Nicolar (1893) does not once mention any other type of leadership than "spiritual leaders," and councils of old men (sometimes only the oldest seven, other times all the elders). "Spiritual power," supposedly to be used only for the general good through priestly ministrations, but capable of being misused selfishly and for evil purposes, was the *sine qua non* of public administration, Nicolar implies (1893:passim).

Because a sagamore-shaman was unlikely to be the only member of his band or tribe to have spiritual power, the presence of another shaman or shamans easily could modify the political influence of the sagamore. Potential competition and conflict between rival sagamores and shamans (each with his faction of commoners) was the result, for better or for worse: autocrats might be controlled, but good leaders might be toppled, thereby.

### *The Sagamore as Opportunist*

By the end of the seventeenth century, French and English contacts had taught Wabanaki sagamores both to master the art of opportunistic exploitation of intruding Europeans and fellow Wabanaki alike and to expect such usage in return. However, interpersonal competition between sagamores for economic, military, religious, and political advantages over rivals is documented from the earliest European accounts. While undoubtedly this rivalry progressed geometrically from initial European contacts onward, it must have had a base in truly aboriginal, pre-contact, Wabanaki culture. How Western Etchemin Bashaba came to be head sagamore of the Abenaki Confederacy we may never know, but the meteoric rise of Micmac Membertou to be called "Grand Chief" of Acadia by the French was purely opportunistic—indeed, it is the first of many well-documented cases of Wabanaki one-ups-manship.

In 1605, when the French decided to make Port Royal (in western Nova Scotia) their major base in Acadia, they gave a sudden once-in-a-lifetime advantage to Membertou, the Micmac sagamore in that relatively back-water area. Both Champlain and Lescarbot knew him personally; the former wrote about him in passing, but the latter described him at some length, as a triple-threat: sagamore, shaman, and *ginap*. Neither Frenchman was blind to Membertou's advantageous use of his French connections.

> We had found him a good Indian all the time we were there, although he had the reputation of being the worst and most treacherous man of his tribe. [Champlain 1613/1922:384]
>
> He has been a very great and cruel warrior in his youth and during his life. Therefore *rumour runs that he has many enemies, and is well content to keep close to the French, in order to live in safety.* [Lescarbot 1618a/1907-14 2:354-355; emphases added]

Membertou borrowed French firearms—in summer of 1607, the first such utilization recorded—to wage an offensive campaign of revenge at the mouth of the Saco River (in southwestern Maine) against some of Bashaba's allies. They had killed a visiting Micmac, apparently after the latter boasted too much about his French friends and tradegoods. Lescarbot (1618b) wrote an epic poem relating this adventure.[16] But despite his military success, Membertou thereafter seems to have retired as *ginap*, probably owing to old age.

In 1610, when the French pitched a religious missionary campaign at the Wabanaki to enhance homefront support for the Acadia colony, Membertou abandoned his profitable practice as a shaman and presented himself and 20 of his kinfolk for conversion to French Catholicism. The ill-prepared secular priest gave them far too little religious instruction, but triumphantly welcomed Membertou as the first native leader in North America to become converted—a distinction highly prized by the wise old sagamore. The group was baptized 24 June 1610.

When Jesuits arrived at Port Royal in May 1611 to missionize the natives properly, they were appalled at the neophytes' lack of preparation for salvation, and immediately set about rectifying the situation. Soon Jesuit Biard wrote such glowing reports about Membertou that one can only conclude that each was greatly adept at opportunistically profiting by the partnership with the other.

Suddenly Membertou sickened with an obviously final illness, and announced that he wished to be buried with his ancestors in a pagan grave. The full truth of what transpired then naturally cannot be filtered through Biard's accounts,[17] but Membertou (or Henri, as he had been baptized) soon was laid to rest in ground made sacred by French religion, not Micmac, in September 1611.

In his *Relation* of 1616, Biard states that "Membertou . . . was the greatest Sagamore, the most followed, and the most feared, that they [the Micmac] had had for several centuries" (Biard 1616 *in* JR 4:103), and "The Savages have no recollection of ever having had a greater or more powerful Sagamore" (Biard 1616 *in* JR 3:205).

So Membertou became remembered, through the teachings of the French missionaries, as the "Great Captain" and the "Grand Chief." The latter term became adopted, in more recent centuries, by the Micmac themselves, who established a nominal "Grand Chief of the Micmac Nation." However, anthropologist Wilson D. Wallis (Wallis & Wallis 1955:176) states: "This office may be no older than the Wabanaki Confederacy (mid-eighteenth century)," and most authorities consider that that alliance (Alliance 2 on the chart) was of French origin, too. Henri Membertou was, indeed, Cohen's *"politically rational man*—one who would rather win than lose," and it would seem that his heroic memory has been exploited by other politically rational men ever since.

### *Sagamores' Role-Model*

It seems fitting to end this description of the status and role of the Wabanaki sagamore as it began, with a word-portrait, but not this time a facetious caricature. As Wabanaki commoners looked to their sagamore, so Wabanaki sagamores must have looked to the Wabanaki culture-hero, Gluskap, for their role-model. Folklorist Charles Godfrey Leland depicts Gluskap as a superman indeed (1884:117):

> Then Glooskap, who was much pleased with all this, for he loved a brave man, came among them looking terribly ferocious; in all the land there was not one who seemed half so horrible. For he appeared ten feet high, with a hundred red and black feathers in his scalplock, his face painted like fresh blood with green rings round his eyes, a large clam-shell hanging from each ear, a spread eagle, very awful to behold, flapping its wings from the back of his neck, so that as he strode into the village all hearts quaked. Being but simple Indians, they accounted that this must be, if not Lox the Great Wolverine, at least Mitchehant, the devil himself in person, turned Wabanaki; and they admired him greatly, and the squaws said they had never seen aught so lovely.

## CONCLUSION

When trying to interpret the ethnohistoric data pertaining to seventeenth-century Wabanaki sagamoreship, much more sense can be made with the use of a suitable model as a frame of reference than without.

This paper has suggested as suitable an eclectic, three-part, super-model, and has used it for the data analysis: Fried's structure model of leadership in the "rank society"; Cohen's motive model of the "politically rational man"; and Malinowski's validation model of the "myth as charter" for double-dealing with "enemies." The three parts in combination seem to yield relatively meaningful (if only tentative) results. As it is merely the pioneering effort in the study of Early Historic Wabanaki leadership, little more should be expected from this attempt. If others can go further, or deeper, hopefully they will.

In sum, the ethnohistorical anthropologist is in a much more advantageous positon to see further into the dynamics of the native world-that-was than is the classic historian, because he can stand upon the shoulders of his elder brothers, the theorists of sociocultural anthropology.

## NOTES

[1] Grateful acknowledgment is offered here for the summary work of Bernard G. Hoffman (1955) on Micmac sociopolitical leadership done as part of his lengthy doctoral dissertation *The Historical Ethnography of the Micmac of the 16th & 17th Centuries.* Hoffman's work is the baseline from which this paper starts, as did also the author's own dissertation (Morrison 1974) *Dawnland Decisions: 17th-Century Wabanaki Leaders and Their Responses to the Differential Contact Stimuli in the Overlap Area of New France and New England.*

[2] An excellent example of ethnohistorical anthropology scoring a point where classical history has not can be found in the recent article "The First of the Mohegans" (Burton & Lowenthal 1974). It systematically elucidates the opportunistic behavior of Uncas, Chief Sachem of the Mohegans, during and immediately after the Pequot War of 1637, in southern New England. This is yet another blow struck against those ethnocentrisms that in recent centuries have so belittled the savoir-faire of "primitive" peoples, in scientific affairs (e.g., Stonehenge) and politics alike.

[3] See the accompanying map.

[4] See the accompanying chart, left-hand column. The difficulties inherent in trying to meaningfully identify who's who, where, and when are considered for Eastern Algonkians in general by Brasser (1968) and for the Wabanaki by Morrison (1973; 1974; 1975).

[5] See Morrison (1974; 1975) for several Wabanaki examples.

[6] See the accompanying chart, right-hand column.

[7] Elman R. Service (1962; 1963) at first used *band, tribe, chiefdom, state,* and *civilization-empire* as both evoluntionary ***stages*** of sociopolitical development and contemporary ***types*** of sociopolitical organization. Sanders and Marino (1970) use Service's terms as evolutionary stages of New World culture-history to A.D. 1500, but combine the fourth and fifth stages under the one term *state.*

Service (1968:166-167) later altered his thinking somewhat. While retaining *band, tribe, chiefdom, state,* and *civilization empire* as contemporary ***types***, he felt that as evolutionary ***stages*** it would be better to think in terms of *egalitarian society, hierarchical society,* and *civilization-empire.* In doing so, Service follows the theoretical lead of Morton H. Fried (1967). Fried (1975) recently has described the *tribe* as solely a reactive creation of imperial influences, either modern or ancient, Western or Non-Western.

[8] Reading even a simple account about the "Mound Builders," such as Claiborne's (1973) chapter thereon, impresses one with the very great extent of either pacified or otherwise-unhostile territory and/or connections required to allow the trading networks to function. Alliances, or at least contracts, between peoples both near and far simply had to have been the basis of such interactions. Tribal confederations easily could arise from alliances, out on the margins of the chiefdoms which must have run the centers of mound complexes. Whether or not one agrees with Fried's (1975) theory of tribal causation, it seems obvious that Meso-American civilization was the *fons et origo* of the Mound-Builder cultures.

[9] What is being implied here is that there seems to have been a traditional Western Etchemin leadership over other Wabanaki peoples of the Maine coast.

Christopher Levett, an English colonial official and entrepreneur, established a trading post at what is now Portland (Maine) on Casco Bay in 1623-24. He had extraordinarily cordial relationships with the area's Abnaki sagamores and their visitors from farther away, and even was adopted as fictive cousin by several sagamores at once. Levett (1628 *in* Baxter 1893:100,111) mentions "the great *Sagamore* of the East country, whom the rest doe acknowledge to be chiefe amongst them." (Apparently he was named Sadamoyt, but Levett's punctuation in the latter reference makes it not quite certain that Sadamoyt and the great sagamore are one and the same person.)

In the last quarter of the seventeenth century, Western Etchemin sagamore Madockawando of the Penobscot Bay area seems to overshadow other Maine coast Wabanaki leaders in relative importance. His daughter married the French nobleman Baron de St. Castin, who became a de facto sagamore of great influence among the Wabanaki, working out of his Penobscot Bay headquarters.

Ethnohistorian Fannie Hardy Eckstorm (1945:79-83) believed that ca. 1725, after the disruptions by the English during the Abenaki War, the first leaders—Neptune by family name—of the Wabanaki heterogeneous tribe now called Penobscot came from that now called Passamaquoddy. The Penobscot were regrouped Abnaki-Pennacook; the Passamaquoddy were regrouped Etchemin.

[10] Further research may reveal that strife between rivals who were kinsmen, while not necessarily common, did indeed occasionally occur among the Wabanaki. Currently there are some hints of this having happened, which need checking out carefully.

[11] Micmac sagamore Membertou's conduct (both in words and deeds) pertaining to his 1607 raid of revenge against the 1606 murderers of his son-in-law is a good case in point, So is the confusing apparent conduct of Abnaki sagamore Sasanou in the events during and/or after Membertou's raid. See Morrison (1975) and Goetz (1975) for details.

[12] A *sanop* was an adult male commoner; sometimes, also, the term refers to one such who is married, as, indeed, the "average man" usually is, in any society.

[13] To date, survey of non-Micmac accounts has produced nothing to contradict Hoffman's Micmac statements. Admittedly, this apparent similarity depends not upon documentation as much as upon lack of documentation. This is what Harold Hickerson (1970:51) calls "that most tenuous and difficult of all historiographic techniques, appraisal of *negative evidence*, which amounts to concluding something did not exist because it was not mentioned." In the present case, the something which does not seem to have existed was non-Micmac Wabanaki difference from the Micmac leadership pattern summarized by Hoffman.

[14] Rosier (1605b *in* Purchas 1905-07 18:358-359) presents a list of words recorded from interviewing the five 1605 Waymouth captives. "This earliest known list of Maine Indian words, says Dr. Frank T. Siebert, . . . is good Abnaki, with a few exceptions" (Eckstorm 1941:250). All five Waymouth captives were Abnaki, "except possibly one, who was perhaps an adopted prisoner", according to Siebert (personal communication).

Given that Bashaba was a Western Etchemin, not an Abnaki, what Rosier (1605b *in* Purchas 1905-07 18:359) says about the five captives is at odds with Siebert's statement: "The Names of our Virginians [Maine was part of generic Virginia, meaning English North America] Bdahanedo, Brother to the Bashabes. Amooret, his Brother. Satacomoah. Maneduck. Scikaworrowse."

[15] Penobscot elder Joseph Nicolar (1893), in his valuable book of folk-history, *The Life and Traditions of the Red Man*, also describes *ginaps* in action, but calls them only "spiritual leaders" engaged militarily. All accounts of *ginaps* imply that all northeastern peoples had such heroes.

[16] See Morrison (1975) and Goetz (1975) for details.

[17] Biard's accounts of Membertou's illness and death all appear in the *Jesuit Relations*. . .: Vol. 1:167,215; Vol. 2:19-25, 97-99,225; Vol. 3:201-205.

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